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A QUARTERLY JOURNAL

OF

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AND THE

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Transactions of the State Microscopical Society of Illinois.

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THE CAUSES OF INFLUENZA IN HORSES.

THE recent appearance of Influenza in horses affords interesting material for study, in connection with the question of the origin of epidemics. Unlike the majority of former epidemics, whose origin has been obscure, this appears to have sprung into existence in the centre of the North American Continent, and in a distinct locality which can be definitely pointed out. It has spread rapidly and steadily in nearly every direction, from this as a centre, and, thanks to facilities afforded by railroads and telegraphs, its course has been traceable with ease. The following is intended as a contribution towards securing the lessons which may be learned from the visitation:

The old doctrine of an epidemic constitution of the atmosphere, has of late years been gradually waning, as cholera, small-pox, typhoid fever, and other epidemics and epizoötics have been traced to more tangible causes, and placed more under human control.

More than any other epidemic malady, perhaps, has Influenza retained its claim on an atmospheric causation. It has been described as falling simultaneously on all parts of a given district, or country, as breaking out in islands a considerable distance from the shore, and without having had any communication with the main land; and as having attacked the crews of ships in mid-ocean, after they had been

twenty days at sea. No wonder that we should have had all imaginable general conditions of the earth, water, and air, by which to explain its occurrence. That at one time it has been attributed to the lowness and dampness of a locality; at another, to the height, exposure, and coldness; at a third, to crowding of population, with the resulting impurities of soil, water, and air; in a fourth case, to the vicissitudes of weather in late spring, in autumn, or in winter, or of some unusually variable seasons; to a persistent low temperature, or sudden variation of temperature; to the prevalence of damp, acrid or fetid fogs and mists; to excessive rain-fall and unusual humidity of the atmosphere; to an unusually high or low density of the atmosphere; to an excess of ozone in the air; to the telluric emanations attendant on great earthquakes and volcanic eruptions, or, to a modified condition of the atmospheric electricity.

The Epizoötic of 1872 affords but the most slender appearance of support to any of these hypotheses. Neither *soil* nor *elevation* has materially affected it. The prevalence and mortality have been almost the same in the mountains of Vermont and New Hampshire, as in the flat and malarious sea coast of New Jersey, Maryland, and Virginia.

The *temperature* has not exerted any marked influence. The disease has been general wherever it has reached, and the mortality has averaged one per cent. or a little over. Indeed, in some cases the comparison has been altogether in favor of the more northern and colder localities. Thus in Fulton Co., Ga., it is reported as universal, and the mortality, up to the date of report, had been one per cent. In Dodge Co., Wis., on the other hand, although after the outbreak of the affection, there had been a sudden transition in a single night, (12 Nov.) from a pleasant Indian summer to the rigorous and persistent cold of winter—the thermometer sometimes marking 8° below zero—yet the losses in country districts are estimated at 1 in 300. Over-crowding, with its concomitants of *hot, damp, vitiated air*, has unquestionably been a main cause of the severity and complications of the disease in the large cities, the pneumonias, pleurisies, *purpura hæmorrhagicas*, &c., but the malignancy of all specific febrile diseases, occurring with such unwholesome surroundings, forbids that we should attach any importance to these, in estimating the causes of this particular disorder. Influenza in man, shows a similar malignancy and fatality in unwholesome

localities, and in over-crowded portions of cities, where hygienic arrangements are imperfect. The observations of Pearson, Parkes, Baker, Gray, and the English Registrar General, have sufficiently established this fact; and in equine Influenza, confined as it often is, to a more limited area than has been the case at present, the affection has been oftentimes limited, almost entirely, to exposed stables, open and swept by draughts of cold air, but with an impure, damp and stifling atmosphere; or close, and without ventilation, light, or drainage. Yet such conditions can only retard or prevent the elimination of effete matter from the system, favor the introduction of the deleterious products of decomposition in animal and vegetable matters, saturate the blood with impurities, and by impairing or suspending nutrition and other important functions, lay the system open to the access of disease. But while they facilitate the development and increase the severity of all zymotic maladies, they do not determine which specific affection shall be developed in a particular case. That is determined by the prevalence of Influenza, glanders, or other specific disorder in the locality at the time, and it is noticeable in this connection, that the equine Influenza of 1872 did not originate in a crowded city, as is generally supposed. Professor Smith, of Toronto Veterinary College, assures me that it existed at a place fifteen miles to the north of that city, before it appeared among the city horses.

Sudden Changes of Weather and Temperature.—Nasal and bronchial catarrhs often prevail extensively among horses as among men in connection with sudden and extreme variations of temperature, and especially in spring and autumn. These are liable to be confounded with Influenza, and hence the idea that this disease is but a simple result of such climatic vicissitudes. In the case of the horse, the changeable seasons are often aggravated by the weakness and susceptibility of the system, in connection with the spring and autumn changes of coat; the transition from the hot stable to the cool field; or, from the clear atmosphere of the pasture to the close, hot, impure air of the stable; the changes from green to dry food, or *vice versa;* and the substitution of work for idleness, or the reverse. That the effects of sudden changes of temperature are very severe on the animal system which has not been habituated to the new condition of life by a gradual transition from one to the other, is well shown in Mr. Edwards' experiments on cold-blooded animals.

Though subjected to a very low temperature in winter, the heat of their bodies declined barely $\frac{4}{10}$ of a degree, whereas, exposure to a cold temperature in summer, insured a depression of body-heat to the extent of 3° and even 6° Centigr. So it is with warm-blooded animals, transferred from a warm to a cold climate. The French cavalry horses, sent from the shores of the Mediterranean to the northern parts of the country, suffer to a great extent from catarrhal and pulmonary affections. But such catarrhal attacks do not spread as an epizoötic, nor extend from the newly arrived horses to those which are permanent residents. Catarrhal symptoms are induced, but the contagion which secures an extensive and general prevalence of the malady is wanting. Such vicissitudes therefore operate like other unwholesome conditions of life; they pre-dispose the system to the disease, or even increase its severity, but they cannot apparently generate the morbid poison.

The first reported cases of the recent epizoötic occurred in Toronto, in the last days of September. It is, therefore, of the greatest importance, to ascertain what was the state of the weather in that locality during the month of September. Through the kindness of Professor Kingston, of the Magnetic Observatory, Toronto, I am enabled to introduce a table, giving the Meteorological Register for the month of September, 1872, at Toronto, and a second table giving the records of the same month for the last twenty-eight years, at the same place. [*For Tables, see pp.* 5 *and* 6.]

From these tables it is manifest that there was no extraordinary condition, nor extreme change in the weather, during the whole of the month, the last days of which witnessed the outbreaks. The mean temperature was in excess of that of September, 1871, but considerably below that of this month, in many previous years. Both maximum and minimum temperatures were slightly in excess of those of 1871, but the maximum is less, and the minimum more, than those of several previous non-influenza years. The monthly range of temperature was nearly 2° less than that of 1871, and 11° under that of 1854, which was not an influenza year. It may be added, the greatest daily range for September, 1872, was 27°5, while for September, 1871, it was 31°5. The greatest velocity of the wind in September, 1872, was 22.4 miles, on the 13th, with wind at the north-west, and temperature 61°4. The highest velocity in September, 1871, was 26 miles, on the 17th, with wind north-west by north, and temperature 52°9.

MONTHLY METEOROLOGICAL REGISTER,

At the Magnetic Observatory, Toronto, Ontario, September, 1872. Latitude 43°, 39′, 4 North; Longitude 5h., 17m., 33s., West.

Elevation above Lake Ontario, 108 feet.

Days.	Barom. at Temperature of 32°. 6 A. M.	2 P. M.	10 P. M.	Mean.	Temperature of the Air. 6 A. M.	2 P. M.	10 P. M.	Mean	Excess of M'n above Norm'l	Tension of Vapor. 6 A.M	2 P.M.	10 P.M.	M'n	Relative Humidity. 6 A.M.	2 P.M.	10 P.M.	M'n	Direction of Wind. 6 A. M.	2 P. M.	10 P.M.	Resultant.	Velocity of Wind. 6 A.M.	2 P.M.	10 P.M.	Result't	M'n	Rain in inches.
1																		NW	NW	Calm	N 57° W	10.0	19.0	0.0	10.64	10.77	
2	29.602	29.640	29.731	29.6703	65.0	70.1	54.2	62.08	— 0.17	.439	.322	.208	.305	71	44	49	53	NW	NW	N	N 31 W	10.4	15.2	12.6	12.13	13.00	
3	.821	.752	.719	.7622	44.8	60.0	50.3	52.05	— 9.93	.214	.281	.230	.220	72	53	63	59	N	SW	N	N 12 W	1.0	3.4	4.2	2.07	3.77	
4	.750	.644	.592	.6375	45.9	60.7	55.0	55.07	— 6.62	.249	.349	.402	.330	80	66	93	76	Calm	S	Calm	S 1 E	0.0	5.1	0.0	1.72	1.98	.150
5	.450	.416	.412	.4273	54.6	66.5	62.9	61.63	+ 0.22	.407	.566	.547	.506	95	87	95	92	NE	E	E	S 72 E	2.5	1.0	1.5	1.34	1.62	.050
6	.416	.452	.457	.4463	62.5	73.7	65.1	66.73	+ 5.68	.566	.675	.585	.598	100	81	94	91	Calm	S	Calm	S 67 E	0.0	1.2	0.0	1.57	1.75	.190
7	.510	.477	.513	.4985	61.4	82.0	73.0	72.85	+12.13	.513	.727	.684	.651	94	67	84	81	Calm	SW	Calm	S 37 W	0.0	12.7	0.0	3.29	3.35	
8																		Calm	W	N	N 87 W	0.0	14.5	8.8	2.90	3.90	.010
9	.718	.738	.751	.7352	64.0	63.6	61.8	63.48	+ 3.50	.507	.512	.469	.500	85	87	85	85	NE	E	E	N 77 E	4.4	8.6	8.2	6.14	6.39	.200
10	.554	.713	.751	.7322	61.4	72.6	63.2	65.27	+ 5.67	.512	.621	.451	.519	94	75	78	83	NE	Calm	N	N 77 E	2.4	0.0	1.0	2.76	3.01	Inap
11	.500	.699	.614	.6822	63.2	71.9	66.5	67.62	+ 8.42	.569	.654	.593	.609	98	84	91	90	Calm	E	SE	S 82 E	0.0	6.0	1.4	1.92	2.02	
12	.234	.301	.391	.4110	66.5	72.6	63.6	67.50	+ 8.68	.580	.631	.537	.596	89	78	92	88	E	S	NW	S 30 E	3.2	10.2	2.5	0.84	3.21	.460
13	.657	.667	.848	.6645	62.5	61.4	52.4	58.17	— 0.25	.526	.412	.327	.393	93	76	83	80	W	NW	NW	N 46 W	13.2	22.4	4.2	12.47	12.68	.050
14	.646	.918	.924	.9117	45.6	63.6	53.1	54.88	— 3.13	.260	.243	.317	.273	85	41	78	66	Calm	N	N	N 2 E	0.0	5.5	8.6	5.14	5.25	
15																		N	S	Calm	N 88 E	3.4	5.3	0.0	1.21	2.76	
16	.718	.584	.548	.6045	45.6	61.8	56.8	56.10	— 1.12	.241	.384	.378	.346	72	70	81	76	N	S	NE	S 59 E	1.2	1.0	3.0	0.51	1.69	
17	.554	.537	.562	.5495	53.5	64.7	50.3	56.22	— 0.57	.355	.418	.321	.366	86	68	88	81	N	SW	Calm	N 68 W	2.0	5.6	0.0	0.39	1.71	
18	.500	.360	.276	.3702	43.0	59.3	54.2	53.85	— 2.52	.256	.405	.389	.364	92	80	92	87	Calm	S	SW	S 27 W	0.0	12.8	4.2	3.05	3.57	.500
19	.234	.287	.510	.3568	49.9	60.4	52.4	53.58	— 2.35	.336	.353	.358	.347	93	67	91	85	NW	NW	NW	N 61 W	0.2	16.0	11.4	7.60	8.09	.508
20	.657	.707	.721	.7020	49.8	61.1	52.1	54.42	— 1.13	.294	.276	.297	.291	82	51	76	70	NW	NW	Calm	N 54 W	1.0	8.3	0.0	7.12	7.51	
21	.646	.524	.486	.5450	48.4	64.3	57.5	57.12	+ 2.00	.294	.387	.418	.381	86	64	88	82	Calm	E	NE	S 87 E	0.0	4.8	0.2	1.19	1.27	
22																		W	SW	Calm	S 55 W	0.8	4.6	0.0	1.73	1.96	.050
23	.741	.686	.708	.7005	59.3	66.9	60.7	62.30	+ 8.03	.503	.518	.497	.505	99	79	93	90	NE	E	Calm	N 79 E	3.5	5.7	0.0	2.68	2.98	.030
24	.733	.693	.561	.6550	60.4	72.3	66.1	67.33	+13.56	.488	.531	.504	.511	93	67	79	77	Calm	E	SE	S 58 E	0.0	3.0	4.4	1.65	1.82	
25	.428	.466	.587	.4985	67.6	65.0	53.1	61.62	+ 8.20	.484	.414	.349	.430	72	67	86	78	S	NW	W	S 25 W	12.5	8.4	2.2	7.30	9.13	.028
26	.578	.470	.342	.4613	42.7	63.6	59.3	56.15	+ 3.17	.234	.337	.394	.325	85	57	78	72	Calm	S	NW	S 26 W	0.0	13.6	17.0	6.75	7.16	.080
27	.511	.575	.652	.5912	44.5	56.8	46.3	48.82	— 3.78	.237	.285	.251	.235	80	50	79	69	W	W	Calm	N 82 W	5.6	13.8	0.0	5.25	5.69	Inap
28	.754	.718	.590	.6860	39.1	56.4	53.5	50.68	— 1.50	.206	.260	.288	.256	86	57	70	70	NW	SE	E	S 61 E	0.6	5.6	12.0	2.25	4.15	
29																		E	SW	SW	S 8 W	9.8	13.8	13.5	7.93	13.98	.220
30	.432	.512	.602	.5292	49.9	57.1	49.5	52.23	+ 0.85	.274	.286	.301	.282	76	62	84	72	SW	W	Calm	S 77 W	2.2	10.3	0.0	8.85	9.18	
	29.6044	29.5838	29.5940	29.5937	54.05	65.13	57.32	59.11	+ 1.86	.382	.431	.404	.406	87	67	83	78					3.00	8.58	4.03		5.24	2.526

COMPARATIVE REGISTER FOR SEPTEMBER, AT TORONTO.

Year.	Temperature.					Rain.		Wind.		
	Mean.	Excess above Average.	Maximum.	Minimum.	Range.	No. of Days.	Inches.	Resultant. Direction.	Resultant. Velocity.	Mean Velocity.
1844	58.6	+0.6	81.8	28.2	53.6	4	Inap.			0.26 lbs.
1845	56.0	—2.0	79.6	34.0	45.6	16	6.245			0.34
1846	63.6	+5.6	84.3	37.3	47.0	11	4.595			0.33
1847	55.6	—2.4	74.5	35.0	39.5	15	6.665			0.33
1848	54.2	—3.8	80.4	28.1	52.3	11	3.115	N71W	2.38	5.81 miles.
1849	58.2	+0.2	80.1	32.7	47.4	9	1.480	N75W	0.69	4.23
1850	56.5	—1.5	76.0	29.5	46.5	11	1.735	S 65W	1.02	4.78
1851	60.0	+2.0	86.3	32.0	54.3	9	2.665	N14 E	1.03	5.45
1852	57.5	—0.5	81.8	35.8	46.0	10	3.630	N17W	0.53	4.60
1853	58.8	+0.8	85.5	33.9	51.6	12	5.140	N	1.06	4.33
1854	61.0	+3.0	93.6	35.8	57.8	14	5.375	N22W	1.33	4.04
1855	59.5	+1.5	82.6	33.0	49.6	12	5.585	N20 E	1.29	7.61
1856	57.1	—0.9	78.4	35.0	43.4	13	4.105	S 79W	1.98	6.53
1857	58.6	+0.6	82.0	34.1	47.9	11	2.640	N68W	1.61	6.55
1858	59.1	+1.1	81.4	35.6	45.8	8	0.735	S 74W	1.53	5.69
1859	55.2	—2.8	75.4	35.7	39.7	15	3.525	N44W	1.60	6.36
1860	55.3	—2.7	75.8	28.7	47.1	14	1.959	N71W	2.63	5.79
1861	59.1	+1.1	78.8	37.1	41.7	17	3.607	N71W	1.39	4.81
1862	59.6	+1.6	79.4	39.0	40.4	9	2.344	N59W	1.07	5.11
1863	55.9	—2.1	80.0	31.4	48.6	8	1.235	N16W	1.92	6.46
1864	56.4	—1.6	73.0	37.8	35.2	11	2.508	N38W	1.89	7.06
1865	64.5	+6.5	90.5	42.0	48.5	12	2.450	S 56 E	0.47	4.12
1866	55.2	—2.8	80.0	34.4	45.6	15	5.657	N33W	1.45	4.63
1867	57.9	—0.1	87.0	31.8	55.2	9	1.226	N37W	1.48	5.43
1868	56.6	—1.4	75.5	36.0	39.5	16	4.239	N74W	0.88	6.68
1869	60.7	+2.7	81.0	34.4	46.6	8	4.027	N53W	1.16	4.89
1870	61.8	+3.8	78.0	45.8	32.2	11	6.794	N29 E	2.26	5.04
1871	54.8	—3.2	81.8	34.0	47.8	8	1.290	N74W	1.72	5.50
1872	59.1	+1.1	84.4	38.2	46.2	16	2.526	N79W	1.47	5.24
RESULTS TO 1871.										
	58.04		80.88	34.58	46.30	11.06	3.716	N52W	1.06	5.44
EXCESS FOR 1872.										
	+1.07		+3.52	+3.62	—0.10	+4.94	—1.190			—0.20

Fogs.—Remarkable *acrid* or *fetid fogs* have been observed to precede or accompany some epidemics of Influenza. Dr. Arbuthnot remarks on the prevalence of such fogs, not only in England, but in France and Germany as well, in connection with the Influenza of 1727, and 1732–3. In the latter year there had been a severe

drought, wells were dry, and from Nov. 4 till Christmas there prevailed stinking fogs, a higher temperature than usual, great storms of wind from the south-east, and lightning without thunder. It was further observed by surgeons, that wounds showed a great disposition to mortify. But in the great majority of Influenza epidemics and epizoötics there has been no such coincidence. The present equine Influenza has neither been preceded nor attended by any such phenomenon. Fogs appeared on but three days, 6th, 11th, and 18th, of September, 1872, whereas they existed on six days, 1st, 4th, 5th, 13th, 16th, and 19th of September, 1871. Fogs or vapors, impregnated with sulphurous gases, or other bad-smelling or putrifying elements, would, undoubtedly, undermine the general health, and favor the diffusion of such a disease as Influenza; but the origin and course of the present epizoötic, like that of the majority on record, shows clearly enough that no such condition is essential to its developement.

Rain-fall and Humidity.—The rain-fall for September, 1872, at Toronto, was but 2.526 inches, as compared with 1.290 inches in September, 1871, and 6.794 inches in September, 1870. The rainy days were 16, in 1872, against 8, in 1871, and 17, in 1861. The total rain-fall in September, 1872, was 1 inch below the average of the twenty-eight preceding years.

The average relative *humidity of the air*, in Toronto, in September, 1872, was 78, against 71 for the same month of the previous year. Though greater than in the former year this is by no means an excess of moisture, and any assumed imputation of this excess will be destroyed by a reference to the following table, giving the relative humidity of the air at other places where Influenza did not appear during September, 1872.

Weeks.	*Toronto.*	*Montreal.*	*Quebec.*	*Detroit.*	*New York.*
1st., ...	73,7	76.5	78.5	70.7	64.4
2d., ...	80.6	79.7	83.0	79.0	79.7
3d., ...	79.5	76.8	82.6	72.0	75.5
4th., ...	77.8	74.7	76.0	66.4	81.0
Av'ge for 4 weeks,	77.9	76.92	80.02	72.02	75.15

The *barometer* had a low average for September, 1872; at Toronto 29.5937, against 29.7200 of the same month in 1871. Its range, too, was less; 0.728, against 0.799 in September, 1871. The average heights of the barometer at Toronto, in June, July, and August, 1871, were respectively, 29.5431, 29.5552, and 29.5780.

Ozone.—It has been strongly contended that this agent is in excess in the atmosphere during epidemics of Influenza. Since Scöhnlein placed a rabbit for an hour in an atmosphere artificially charged with *ozone*, and found a resulting inflammation of the mucous membranes, and the death of the subject a few hours later, the potency of this agent in causing Influenza has been largely assumed. Additional weight was given to the theory by the observations of Bœckel, of Strasbourg, who found that an excess of ozone in the atmosphere, if associated with cold, east or north-east winds, or snow, was capable of inducing inflammation of the air passages. Bœckel further found, that when he compelled animals to breathe strongly ozonized air, lobular pneumonia was produced.—(*Levy.*) But there is no evidence that the catarrhs and pneumonias thus produced were capable of extending, and assuming the character of an epidemic.

It is found, indeed, that ozone does not exist in an atmosphere loaded with organic impurities, the product of decomposing material, or of animal respiration. Birinez could find no indication of the presence of ozone in the surgical, fever, and venereal wards of the Military Hospital at Versailles, though it was abundant in the court-yard of the hospital. James found a great deficiency in the Military Hospital at Sedan as compared with the garden of the hospital. Bœckel found it abundantly on the platform of the cathedral at Strasbourg during the prevalence of cholera in that city, but he rarely found a trace in the streets of the town. He further asserts as the results of his observations, that in air charged with paludal emanations ozone is not produced. He was, moreover, unable to develope ozone to any extent in a cholera ward.

But these are precisely the conditions in which Influenza assumes its greatest severity and shows its highest death-rate. In the large cities where the air contains an excess of carbonic acid, resulting from combustion of fuel, animal respiration, and an abundance of decomposing organic matter, the products of waste and decay in organized bodies, this is unquestionably the case. And just in proportion to the squalor, the filth, the impurity, and the absence of a proper hygiene, so does the affection prove more severe and fatal. So it is in the close, unventilated, undrained, or underground stables of cities, with air loaded to suffocation with the products of respiration and putrefaction. In these the mortality proves far in

excess of that of the horses in the better appointed stables, or in the country. A review of the whole subject shows very conclusively that an excess of ozone in the atmosphere cannot be accepted as the one cause or the main cause of Influenza.

Again, it is difficult to estimate the amount of ozone in the air. Nitrous acid, which often exists in great amount near the surface of the earth, which, like ozone, is produced in large quantities during thunderstorms, and like it decomposes organic matter in the air, has precisely the same reaction as ozone on iodized starch papers. Ozone, moreover, is always present in larger amount at the higher altitudes, but Influenza shows no such predilection for the hills. It has on the other hand, during the recent epizoötic, shown a decided preference for the valleys along which run the great railroad tracks, as evinced by its earlier *dèbut* at such places.

Once more, the amount of ozone varies constantly on the sea-shore, from the great evaporation and the everchanging condition of the electricity, and a sea-side residence has been accordingly advised as a safeguard against the evil effects of an excess of ozone. But the recent epizoötic had its origin near the borders of a large lake, and has in the main prevailed earlier and more severely in the large towns on the Atlantic sea-board than in the inland districts. As examples, may be mentioned New York, Brooklyn, Jersey City and Boston, attacked on or about Oct. 22d; Portland, Me., Newport, R. I., and New Haven, Conn., on Oct. 23d; Portsmouth, Va., Nov. 1st, and Charleston, S. C., Nov. 2d, whereas it only appeared at Kingston, N. Y., on Nov. 1st, and at Scranton, Forrest Co., Clearfield Co., and elsewhere in Pennsylvania, about Nov. 14th.

Dropsies and other dangerous complications were also very prevalent in these sea-port towns.

I have not been able to obtain ozonometric observations made during the epizoötic, but beside as the above considerations, the evidences of the transmission of the disease by *contagion* may be adduced as disproving its pathogenisis and propagation by ozone.

Electricity.—As in the case of ozone, no reports of the state of the atmospheric electricity are available, but like ozone, if potent at all, this agency could only be so in producing the first case or cases. It might be conceived of, as affecting the nutrition of the animal body, so as to produce from its elements a morbid poison capable of indefinite reproduction, and of communicating the disease from animal

to animal. But to conceive of the same electrical condition spreading by slow and regular advances over the greater portion of the continent for the space of three months, in all the varied phases of hill and plain; of rain, snow, and fair weather; of clouds and sunshine, of atmospheric moisture and dryness; of storm and calm; in city and country; on the inland table-land and valley, and on the sea-shore, is not in keeping with what we know of this agency.

According to Peltier the electricity of the earth is always *negative*, and that of a dry atmosphere *positive*. Guy-Lussac and Biot found that the greater the altitude they attained in a balloon, the stronger was the positive electricty. Becquerel and Breschet found no evidence of positive electricity in the six feet nearest the surface of the earth, in close-sheltered places in the court-yards of houses, in the streets of cities, or in narrow valleys. In a calm, pure atmosphere the electricity is uniformly disseminated and therefore little marked, but with a lowering of temperature and the condensation of the contained watery vapor into more or less dense clouds, the electricity concentrates itself around the watery particles and leads to extensive disturbances of the equilibrium. The action of the earth renders these clouds more negative in their upper than in their lower portions. Water falling in rain is as often positive as negative, falling as snow it is positive four times in five. Slight rains do not modify the atmospheric electricity, while heavy rains increase it positively or negatively. The approach of a hailstorm determines great irregularities in the electric tension of the air. Strong winds also seriously disturb the equilibrium. It has been stated that rains occurring during south, southeast and southwest winds are mostly negative, while those with north, northeast and northwest winds are oftener positive.—[*Levy.*]

Setting aside the regular diurnal variations, it follows, that in the same latitude, location, the proximity of trees or buildings, the force and direction of the prevailing winds, the existence or non-existence of clouds, and the occurrence of heavy rain, hail or snow mainly affect the atmospheric electricity. Some approximation to the electrical disturbance might therefore be attained by noting some of these conditions during the month. The resultant direction of the winds during September, 1872, at Toronto, was N. 79° W., and in September, 1871, N. 74° W. The mean velocity for the month was 5.24 miles per hour in 1872, and 5.50 miles per hour

in 1871. The maximum velocity in September, 1872, was 29 miles, in 1871, 26 miles. In September 1872, twenty days had each a less average than 6 miles per hour, while ten days each averaged from 6 to 10 miles. In September, 1871, eighteen days individually averaged under 6 miles per hour, while twelve days had averages ranging from this up to 10 miles.

Rain fell on sixteen days of the month in 1872, the total duration of fall being 43.4 hours. It fell on eight days in 1871, the duration of fall being 27.7 hours.

The observations made thrice daily in September, 1872, at Toronto, report the weather in 29 instances cloudy, 3 times hazy, 1 time foggy, 3 times threatening, 5 times a light rain, and 1 time a heavy rain; it is 26 times reported clear. It was noted calm on 28 occasions, 7 times calm and clear, 14 times calm and cloudy, and 7 times calm and foggy.

It is manifest from these data that there must have been considerably more disturbance of the electrical tension during September last in Toronto than during the same month of 1871. And the frequency of thunder and lightning testifies to the same truth. September, 1872, had thunder and lightning on the 5th, 6th, 7th, 8th, 12th, 18th, 19th, 22d, 23d, 26th and 29th. September, 1871, has thunder reported on the 3d, and thunder and lightning on the 18th. I have not before me the report of the thunderstorms at Toronto for the earlier months of 1872, but for 1871 there were but 6 storms reported for July, 6 for August and 3 for June. Altogether there appears to be testimony to the existence of an unusual amount of disturbance of the electrical equilibrium for the month of September, 1872. But whether this is sufficient to account for the *origin* of Influenza may still be disputed. It is needless to deny how man and beast often suffer during the prevalence of the electrical disturbances, and especially just before the bursting of a thunderstorm. And considering how the nuclei (nutrition centres) of the different animal tissues have their functions arrested or perverted by inflammatory action, and considering further the varied development of many of the lower organisms, when placed in different circumstances, it does not seem very irrational to assume that under varying conditions of electrical action and of other attendant circumstances there may be developed from these ultimate living particles of the animal body, or from vegetable organisms, new organic particles, with novel and

pathogenic properties, capable of multiplying indefinitely, and disseminating a specific disease.

But there is no evidence that this is really the case. We have merely the coincidence of extensive electrical disturbances and the outbreak of the influenza of 1872. With regard to former epidemics, Dr. Parkes says that "no evidence has been collected which shows any connection with conditions of telluric magnetism, or atmospheric electricity, and indeed the peculiar spread and frequent localization of influenza seem inconsistent with general magnetic conditions." And how often do we see thunder-storms occurring day after day for a length of time without the supervention of influenza!

It is not at all improbable that this electric condition of the atmosphere had something to do with the development of the epizoötic, but in view of all the known facts, and of our experience of the past, we can only look on it as predisposing the system to the attack of a poison, which previously existed, but had remained latent for want of a receptive subject. Considering the feverish condition of the system in times of great electric tension, the amount of ozone resulting from electric discharges, and the known action of ozone on the respiratory mucous membrane, the doctrine is at least plausible, that the diseased condition and lowered vitality of this membrane at such a time lays it open to the attack of the poison. But in support of this theory, as *invariably* operating, we must assume, either the supervention of this electrical derangement at each place whenever an animal is attacked, or that the reception of the poison into the animal body changes its character and intensifies its virulence. This gradual march of the electric tension over the continent seems an extravagant and unwarrantable assumption. The acquisition of increased potency or virulence by passing through an animal body, is not altogether incompatible with what we know of the varied developement of some of the lower forms of animal life in different media.

It will be observed that this hypothesis of the etiological importance of electricity and ozone, does not touch the question of the primary origin of the poison. It assumes the poison to be already in existence, and that these agencies merely lay the system open to receive it as do impure air, exhaustive, unsuitable food, and other health-depressing causes.

Whatever the significance of the electrical disturbances at Toronto, in September, the fact ought to be recorded for the guidance of future observers.

Progress from East to West, or from West to East. The old doctrine was, that Influenza always extended from east to west, as it had been repeatedly traced over Asia, into and through Europe. The epidemics of 1781, 1800, and 1833, were remarkable examples of this. Yet it has often followed an opposite course. The epidemic of 1768 prevailed in America before it reached Europe, and Webster claims the same course for those of 1757, 1761, and 1781. Gluge, from an induction of all the epidemics known to have occurred for 300 years, concluded that the general course was from west to east. The recent equine Influenza has spread from Toronto in a direction east, west, and south, and indeed, any conclusions based on the direction pursued by the malady must be given up.

Contagion.—Is there a specific *contagion?* This is manifestly a question of vital importance with reference to the etiology of the disease. If there is a *contagion* which may exist in the body of the sick animal, increase there, and be the means of communicating the malady to an indefinite number of sound stock, all our theorizing on noxious gases and putrid fogs, inclemencies and extreme vicissitudes of the weather, excess of ozone, magnetic disturbance, and the like, will be of small account. Indeed, no one of the conditions we have been considering, nor all of them put together, can explain the regular progress of the recent epizoötic, step by step, from a given point of origin, over the whole Atlantic Slope of the Continent, extending over a period of now near three months, and without being naturally influenced by locality, soil, altitude, weather, or climate. No such condition will explain the fact that horses only have suffered, while all the animal creation beside have escaped. In other great epizoötics man has often suffered at the same time with the horse. If these resulted from atmospheric causes alone, how comes it that man has escaped now? The explanation would be easy if the equine and the human malady were alike due to specific *contagia*, distinct from each other but closely allied in their manifest results, and in the conditions which favor their local development, or reproduction.

Were the morbific agent a simple gas, it would be excessive in amount and easily appreciable at the point of origin; it would continue to exert its influence at this point if its production lasted; it would not expend its power there and advance by successive steps over newly conquered territory, each to be as promptly relinquished

in its turn; and, unless uniformly diffused through the atmosphere, and in all parts of the globe, it would be speedily diluted and rendered inert as it spread from its centre of origin.

The same remarks would apply to putrifying organic matter in the atmosphere. This would soon be changed by the action of oxygen into new compounds and lose its original properties. It would be easily appreciable in the atmosphere, and would soon expire by its own limitation, and by the completion of the putrefactive process.

The only theory that will accord with the history of the malady and its steady increase and extension, is that which recognizes the existence of a *contagion* capable, like other specific disease poisons, of assimilating its appropriate food, of re-producing its elements, and of thoroughly increasing the area of the disease.

The history of the recent visitation shows an unmistakable tendency on its part to progress most rapidly along the lines of commerce and travel. It broke out near Toronto, Canada, in the latter part of September, was reported in the city on October 1st, and prevailed in Montreal and generally in the Canadas on October 18th. On October 13th it was known to exist in Detroit; on October 14th in Buffalo; on October 17th Rochester had half its horses sick; on October 19th Lockport, Canandaigua, Geneva, Syracuse, and Albany, were reported attacked; while Batavia, Auburn, and Utica were still reported sound. On October 22d it had reached Revere and Boston, Mass.; Lewiston, Me.; and New York, Brooklyn, and Jersey City. Yet Poughkeepsie was only attacked on Oct. 27th, and Kingston, Duchess Co., N. Y., on Nov. 1st, though apparently in the direct line of the atmospheric wave, had such there been. It reached Philadelphia on Oct. 27th, Washington on Oct. 28th, Columbus, O., on Oct. 29th; Cleveland, Ohio, on Oct. 30th, and Pittsburg, Pa., on Oct. 31st; Norfolk and Portsmouth, Va., on Nov. 1st; Goldsboro, N. C., and Charleston, S. C., on Nov. 30; yet it only reached Binghamton, N. Y., on Oct. 28th; Ithaca, still further north, on Oct. 31st; Titusville, Pa., and Nyack, N. Y., on Oct. 29th; Kingston, N. Y., on Nov. 1st, and Scranton, Pa., on Nov. 13th.

Not only do we find a tendency to follow the great lines of rail, but in many cases a temporary avoidance of many of the smaller towns on the track whose commercial relations are less active and whose danger of infection is accordingly less.

It only remains to be determined whether the disease will spread in a new locality from a newly imported sick animal as a centre. If it can be introduced in this way into a new locality well out of the former area of the disease, and spread promptly from the imported sick animal as a centre, it must be possessed of a specific *contagium*. Were the body merely charged with noxious gases, with decomposing organic matter, or with electricity, it could never become the centre for a wide diffusion of a specific disease. These agents would soon pass from the system and lose their noxious qualities by diffusion or decomposition. The presence of a sick animal would be no more injurious than a chemical laboratory, a putrid carcase, or an electric machine.

Attention is called then to the following facts: The first cases in Detroit were in several horses imported from Canada, about the 10th or 12th of October, and which were noticed ill on the 13th, and the malady appears to have been confined for nearly a week to the two stables into which these horses were taken. The first cases in Syracuse were newly arrived Canadian horses. The earliest cases which I have been able to trace in Ithaca, were in the livery stables of Mr. Jackson, who had just returned from running a mare in a more Northern part of the State. In Pittsburg, the disease first appeared in the livery stables of Messrs. Moreland and Mitchell, after the arrival of five or six horses from New York City where the epizoötic was then at its height. In each case it rapidly spread through the city. From Washington the first note of alarm was sounded on October 28th, to the effect that sick horses had been brought into the city from the North, and on October 31st, it was reported to be generally prevalent. In Lehigh County, Pa., it appeared about November 4th, and "spread like fire along the canal and into the surrounding country." On November 19th, it was reported at Giles, Rutterford, Maury, and other places in Davidson and Summer Counties, Tenn., that had been visited by a circus which came from an infected locality. Several of these were instances of the appearance of the disorder in an entirely new locality, far beyond the limits of the region formerly pervaded by the disease, and from such new points the affection spread widely, before the general country or many of the towns in the interval between there and the former diseased area, were involved. Instances of the same kind could easily be adduced from the history of former epizoötics.

In influenza in man, similar observations have been made by such authorities as Barker, Haggarth, Williams, Parkes and Sir Thomas Watson. Persons just arrived from an infected place have so frequently proved the centre for a new diffusion of the poison, that some have attempted to trace all cases to contagion alone.

It will be objected to this doctrine that Hertwig's inoculations, and even the transfusion of blood from a sick to a healthy horse, have failed to transmit the disease. In the face of such testimony as is furnished above, the conclusiveness of this evidence may be safely denied. Every individual is not susceptible. I can point to horses which have been freely exposed in the streets, and have even stood in the stalls just vacated by sick horses, and have yet completely escaped the disease. The argument from transfusion is no more conclusive than was the failure of the blood of cholera patients to induce in healthy men. It does not disprove the existence of a poison, but merely that the subject was an insusceptible one; or that the poison is not present in the blood.

Nature of the Contagium.—The existence of a *contagium* being acknowledged, the question next arises as to its nature. We are left to choose between two theories. 1st. That which recognizes in fungi and other low organisms the specific poison; and 2nd. That which seeks the pathogenic element in the infinitesimal granules of organic matter found floating in the infected atmosphere, as well as in the solids and fluids of the diseased body. The first named theory is liable to the objection that no *specific* vegetable germs have been found in the air, blood or nasal discharges during the prevalence of the influenza. Before the advent of the recent epizoötic at Ithaca, the writer subjected the floating elements of the air obtained in stables and fields, to microscopic observation, and repeated the observations while the affection was advancing to a climax. Spores were found abundantly, but of the same kind before and after the arrival of the disease. The mucus from a sick horse's nose contained similar spores, and the dust obtained by shaking a little hay contained them in great abundance. This conclusion is fully corroborated by the observations of Dr. Woodward, on the air of stables and the morbid discharges from the nose.

The other doctrine is the most reasonable one, and is one which appears to explain all the pathological phenomena. It recognises in the granules, which exist abundantly in the diseased organs, the

morbid agent capable of transmitting the disease. Those granules which are merely microscopical particles of variable size and form, often possess many of the characters of the ultimate nutrient centres, (*nuclei, germinal matter,* of Beale,) even to the power of absorbing coloring matter, which seems to imply their power of appropriating other material and of increasing or multiplying their substance. These nuclei or granules are reproduced with extraordinary rapidity in the substance of the diseased mucous membrane, and at the expense of the vital elements, liquid and solid, of the body, so that Beale and others have concluded that they either constitute the virulent principle or contain it. Yet nuclei or granules increase to an extraordinary extent in parenchymatous organs, the seat of simple inflammation. These of course cannot be considered as pathogenic. And yet there is no greater reason for assuming a similarity of developmental power in these nuclear products of a simple inflammation, and those of an Influenza or Rinderpest, than for assuming equal powers of growth in the nuclei of different healthy organs and structures. If nuclei, apparently indistinguishable from each other in all respects except their position, never fail to build up the substance of that particular tissue to which they belong; the nuclei of bone invariably producing bone; those of gristle, gristle; those of fibrous tissue, fibrous tissue; those of muscle, muscle; and those of nervous matter, nervous matter; and if we can engraft the nuclei of bone and other tissues so as to build up such tissues in unusual situations, is there any objection to the conclusion that one class of such morbid granules are harmless, while another class invariably develop Influenza and that alone, a third class small pox and that alone, a fourth class glanders and nothing else, a fifth class Rinderpest only, a sixth the contagious lung-plague of cattle, and so on. The physiologist has learned to realize that living particles which are almost infinitesimal in their minuteness have character as constant and a power of development as certain and definite as the genera of animals from which they were derived.

There is no valid objection, therefore, to the theory which recognises in these products of a specific disease, the virulent elements by which the affections are perpetuated and transmitted. And this is the theory which at the present time appears to be most in accordance with the history of Influenza.

In taking this position it is not sought to deny the conveyance of the diseased product by atmospheric means. The numerous instances of horses having been attacked in the open fields apart from all roads and from other horses, and the rapid diffusion of the disease over a city or district, seem to imply the intervention of the atmosphere. But the position assumed by no means precludes such an agency. It only assumes that there is a specific virulent element, which finds in the body of a susceptible animal the material essential to its growth, its unlimited reproduction and its extensive diffusion. The air may still be invoked as an important medium through which the dried and drying virus or *bioplasm* (Beale) may be carried to long distances, to infect new animals and localities. It is further in keeping with the theory that the skin and clothing of human beings and solid objects of nearly every kind may become the medium through which the disease is conveyed from place to place, and would thus explain many outbreaks which would otherwise appear spontaneous.

This theory further explains the outbreak on islands near the shore simultaneously with its appearance on the mainland, and all well authenticated cases of the infection of ships' crews at sea. Thus the Equine Influenza is alleged to have appeared on Block Island, above ten miles at sea, on the same day that it broke out on the Connecticut shore. Were it proved (which, however, has not been attempted) that there had been no recent communication between the island and the shore, there would be nothing in the fact to overthrow the position taken in this paper. A similar case is that of the Stag frigate recorded by Watson. In 1833 this ship was coming up the English channel, and when off Buckley Head, in Devonshire, the wind blew strongly from the shore at 2 o'clock, at which time all the men were healthy (and it is presumed, but not affirmed, that there had been no communication with the shore); at half past two, forty men were suddenly seized with Influenza, at six o'clock sixty men were ill, and by next day one hundred and sixty.

The instances of Admiral Kempenfeldt's and of Lord Howe's Squadrons, attacked while cruising at different parts of the same channel, in 1782, after they had been from twenty-two to twenty-seven days at sea, are no more difficult to explain. Indeed, the fact that a squadron had been technically a number of days at sea, is no proof that officers and men had not availed of their near proximity to pay frequent visits to the shore.

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ARCHEBIOSIS AND HETEROGENESIS.

In investigating a subject so difficult as "The Beginnings of Life," or "Archebiosis," as Dr. Bastian calls it, we cannot be too careful in experimenting, or too cautious in interpreting results. Dr. Bastian very properly insists upon this, when criticising the views of the "Panspermatists," and himself devises certain crucial experiments, so they appear to him, and at present we feel disposed to admit the conclusions to which he has arrived from these experiments, viz., that life can originate *de novo*, so far as *bacteria*, *torulæ*, and certain minute algoid filaments are concerned; but of all which we have only the slightest knowledge; very little, in fact, except that certain minute things, apparently living, and called by these names, do really exist. Granting, then, for the present, that Dr. Bastian has made out a clean case, as to the *de novo* origin of these organisms, we must plant ourselves just here, and affirm that he has not proved anything beyond this; all the wonderful changes and transformations, or at least the majority of them, as detailed in Part III of the second volume of his "Beginnings of Life," under the name of "Heterogenesis," are simple conjectures, as he will find when he subjects these claims to such critical tests as he has those of Pasteur, and others of the "air germ" school. This is the more unfortunate, because the perusal of the first volume of Dr. Bastian's so far really excellent book, led us, as doubtless it has others, to believe that the same care and conscientious search for truth would characterize the whole. But now, when we find so much of manifest error, in ground which, for fifteen years past, we have carefully and patiently worked over, how can we help feeling somewhat shaken as to the truth of the results which Dr. Bastian has arrived at in those parts of his book in which he treats of subjects with which we are less familiar? If Dr. Bastian, or any one competent, "will but devote two or three months to the careful study of the changes which" (he supposes) "are apt to take place in the substance of many of the fresh water Algæ, or in those beautiful green animalized organisms known by the name of *Euglenæ*, some of whose marvellous transformations" (as the Dr. asserts) "were faithfully described more than twenty years ago in the highly valuable, but much neglected memoir of Dr. Gros," then he, or any other competent observer,

will find that there is no proof for the majority (if not for all) of these "marvellous transformations"; and Dr. Bastian will yet learn that he would have produced a better book if he (as well as others) had neglected the memoir of Dr. Gros.

That it may appear plainly what is Dr. Bastian's real belief, for he himself confesses that he has not witnessed actually these wonderful transformations, though he does not doubt them, "rashly trusting" to his "own theoretical convictions," a freedom for which he rightly blames others,* we quote verbatim from the Index to the two volumes: "Desmids, convertibility of into Diatoms or Algæ, ii, 455. Diatoms, origin of, ii, 412, 416, 418, 441, 444, 453; terminal forms of a divergent series, ii, 455; Euglenæ, transformation of into Diatoms, ii, 441, 444, into Desmids and Pediastreæ, ii, 446." We propose to examine somewhat critically the places indicated above, as well as others, principally from Dr. Bastian's second volume, in which at p. 455 we find the following passage: "It seems, however, to be quite certain that a community of nature exists between Algæ, Pediastreæ, Desmids, and Diatoms, since similar vegetal cells *may*, on the same or on different occasions, grow into forms belonging to either one of these groups; and, moreover, the forms *are strictly convertible with one another* until they *chance to assume the forms of Diatoms.* * * Diatoms constitute the terminal forms of a divergent series. The middle terms of the series, however, viz., Pediastreæ and Desmids, are convertible in both directions, either back into Convervæ or onwards into the *less vitalized Diatoms.*" The italics are our own. Now here is a distinct assertion; but, as we shall see, it is simply an assertion, supported by no real proof. Dr. Bastian knows very little about Diatoms or Desmids, and deals with them altogether at second hand, and from very doubtful authorities. As to the less vitalized character of Diatoms, and their *chancing* from Pediastreæ and Desmids, no one at all familiar with them in the living condition can for a moment believe it. They have a far more complicated internal structure than the more highly vitalized (!) Pediastreæ and Desmids, from which, according to Dr. Bastian, they may chance to assume their forms. We have observed the growth and reproduction of Diatomaceæ to little purpose, according to Dr. Gros and Dr. Bastian.

We have witnessed more of the phenomena of conjugation and growth, probably, than any other person, and can affirm, without

*Preface, Vol. 1, p. xii.

fear of its being disproved, that such chance, or indeed any kind of transformation of Pediastreæ or Desmids into Diatoms, never has happened, nay more, never will happen. Dr. Bastian has never seen it, and as for Dr. Gros, well, twenty years ago men might be pardoned for believing many things which we smile at now. When Dr. Bastian, or any competent observer, watches the transformation *through every stage*, and no link of the chain is missing or defective, then, and not till then, can we believe it. It will not do to take for the same things in different phases of development, certain microscopic appearances of agreeing size, form, or place. What we insist upon is the positive proof; and that Dr. Bastian has been misled by appearances (and by Dr. Gros), or to use his own words nearly, that his "presumptions have stolen a march upon established facts," will, we think, be tolerably evident as we explain the real significance of some of the appearances actually observed by Drs. Bastian and Gros.

Passing by Dr. Gros' own words, quoted by Dr. Bastian, we come, on pp. 414, 415, 416 and 417, to the actual observations of the latter. We will not question now that part which relates to the production of "unmistakable filamentous Desmids," (though there is no proof of their Desmid character, other than a remote resemblance), we look more particularly to the account of evolution of Diatoms, fully convinced, however, that the errors in misinterpreting what he saw, are quite as great with the Desmids as with the Diatoms. The wood-cut, p. 417, fig. 82, entitled, "Modes of Origin of Desmids and Diatoms," has, by way of explanation, "*e e'*, * * * * "Pediculated Diatoms were also seen budding from the same *Cladophora* filament." Poor as the cut is, we easily recognize these "pediculated diatoms" as *Achnanthes exilis in its normal condition;* and, if Dr. Bastian wishes, we can show him thousands of this well known form, pretty much as he figures it, growing on a pedicel, the *result of its own secretions*, not only on *Cladophora*, but quite as frequently on *Mougeotia*, *Vaucheria*, or some other fresh water Alga. The marine forms, *Achnanthes longipes*, *A. brevipes*, *A. subsessilis*, &c., all attach themselves by a similar stipes, to marine confervæ. *E. g.*, we have *A. brevipes* by us now, abundantly, on *Ectocarpus siliculosus*.

What is represented by Dr. Bastian, then, is no process of *budding* at all. The little diatom in question, *A. exilis*, we have found con-

jugating, and it differs in no wise, in its life history, from the larger well known forms. Any one who will observe the large and living Diatoms with care, will notice the nucleus and ramifying nerve-like threads, and the beautiful distribution of endochrome with reference to these. These have been partly figured by Prof. Max Schultze, in Müller's *Archiv.* 1858, Taf. xiii, and copied (not equal to the original) in the *Microscopical Journal,* vol. vii, pl. 2.

In addition to the nucleus and ramifying threads, many Diatoms exhibit a germinal dot, with reference to which the endochrome is arranged, rather than to the nucleus; particularly is this the case with *Surirella.* We may add, that the colored figures in Smith's *British Diatomaceæ,* almost without exception, are caricatures; indeed, the late Tuffen West admitted to us, that some of those representing conjugations were manufactured to order. We assert then, that the little "budding" Diatom figured by Dr. Bastian, fig. 82, is growing quietly, after the fashion of Diatoms, a direct result of self division of some former *A. exilis,* and so back, to a sporangial frustute; and that, if it had been allowed to live, it would have continued the process of self division, until finally, at the proper season, and under proper influences, a new sporangium would have been formed, the commencement of a new series, in all respects, however, like the normal form; and that no transformation of *Euglena, Pediastrum,* Desmids, *Vaucheria,* or *Cladophora,* is ever in any way connected with it. We have by us now a gathering of this Diatom with conjugating forms, and the process is entirely similar to what we have witnessed in the marine forms belonging to the same genus, as well as to Diatoms in general.

With regard to the marine forms, which are far more numerous than those of fresh water, we might ask, where did *they* originate, or rather how become terminals of a series, with Pediastreæ and Desmids for middle terms? since, if we mistake not, these middle terms are seldom, if ever, found except in fresh water! Perhaps this might not appear to be much of an objection, inasmuch as some species affect equally well fresh and salt water, but if we get the gist of Dr. Bastian's argument, he would not only have us believe that *bacteria,* &c., originate *de novo,* which at present we grant, but that somehow (the way not yet proved), say fungus spores, *Euglenæ, Astasiæ, Actinophrys,* or something else, come from these "first beginnings"; and next, that somehow, not yet shown how, Pedias-

treæ and Desmids, and finally, Diatoms, come from the previous existing organisms, all a series of transformations, not effected once for all, but *continually going on;* so that all these things are being manufactured, as it were, every day.

Doubtless *bacteria* were developed at a very early period of the earth's history (Dr. Bastian informs us, and we have no desire to question it, that they soon make their appearance after a prolonged boiling of the infusion), but somehow these primæval "beginnings" appear to have been very chary of evolution, as neither Diatoms or Desmids appear earlier than the Creteceous, or what is far more probable, the Tertiary. Somehow, through all this long period, they behave just as we have always found them to do now, viz., not long after their appearance, die, or at least become quiescent; and if other organisms appear where they were, or among them, it is by no means proved that these are transformed *bacteria*, or *torulæ*, or anything similar. And while upon this subject of the first of appearance of *bacteria*, we may be permitted to ask, why, in watching their development in their films of fluid, beneath a covering glass, after it had been cemented to the glass slip, it is necessary, as explained in the foot note, vol. i, p. 294, "to leave a minute aperture at the circumference of the glass uncovered by the cement"? Is this for admission of air germs?

We resume our consideration of figure 82 and the explanation. Certain algoid vesicles, budded (probably like *A. exilis*) off from *Vaucheria*, "gradually become converted into different kinds of Diatoms! (*l l'*, *m m'*)." With reference to these algoid vesicles, Dr. Bastian states, vol. ii, p. 416, that "These bodies increased in size, and it soon became obvious that they were young *Naviculæ* (*l l'*). The exact pattern assumed in the early stages is subject to much variation, and several different Diatoms *seemed* (italics ours) to be produced corresponding to these different initial forms (*m m.'*)" This would be wonderful, *if true;* but, not only is there no evidence that actual Diatoms *did* come from the vesicles of *Vaucheria*, but any one familiar with the observations of living Diatoms can tell *where* they did come from. We venture to assert, that not a Diatom observed by Dr. Bastian came from the vesicles in question, but that they, or their immediate progenitors, were in the gathering which contained the *Vaucheria*, and made their appearance out of the *débris* and general mass after a little period of quiet, as we know

they will do under influence of light (of which something more presently). But besides, Diatoms *do not grow by increase of size*, there is no such thing as broods of young frustules, as the late W. Smith and others have supposed; they generally diminish by continual self-division, or at least continue of the same size, as we have abundantly proved. The late Dr. Greville, a recognized authority as to Diatoms, fully agreed with us as to this. We are not disputing that Dr. Bastian saw these minute and various little *Naviculæ*, but we do say, he is building his theory upon what *seemed* to be, not on what really were, the facts. The influence of light and quiet in bringing these little forms out of their recesses in the *mud*, was well illustrated in an experiment we once performed. An immense number of minute *Naviculæ* were very carefully scraped off the blue mud of a river bottom, in shallow water, and transferred to a phial; of course, though as great care as possible was used to get them pure, the mass when shaken up appeared quite slaty. Observing that a leaf, when lifted from the hard bottom, left its form outlined distinctly, the Diatoms coming up to the light all around it, we tried the following experiment: The mud (and Diatoms, together a slate color) was spread in some thickness upon a strip of glass, and a number of pieces of moistened blotting paper laid upon it. The slide was then turned over, and a pattern (lace) placed on the glass, and the whole exposed, as in printing a photograph. In something like half an hour the pattern was removed, and the outlines were distinctly shown by the little Diatoms coming up towards the light. It is a quite common dodge to separate, and get Diatoms pure, by exposing the material containing them to a strong light, in a saucer under a glass cover; and, if Dr. Bastian wishes, we can show him many excellent specimens thus prepared. We proceed, however, in connection with the appearance of these "young (?) *Naviculæ*," to refer to figure 84, *q*, vol. ii, where, in explanation, it is stated, "*q* Resolution of *Euglena* into Diatoms"!! In the text, however, the author says, "I have only distinctly observed appearances *indicative of this transformation on one occasion*, but in this case, the whole of the contents of a Euglena *seemed* to have been resolved into seven distinctly striated *Naviculæ*. * * *Although the earlier stages of the transformation were not seen* (italics ours), I have no doubt that the Diatoms originated in this way." Vol. ii, p. 441. The Dr. is more easily satisfied that a *Euglena* can transform into a Diatom,

which possesses a wonderful, siliceous, and beautifully sculptured epiderm, than he is that *bacteria* come from air-germs. It will not do to trust "the misguiding influence of a treacherous analogy" in Dr. Bastian's case, more than in that of the Panspermatists, and to decide that, because these seven *Naviculæ* were in what appeared to be the thickened envelope of a *Euglena*, about the size of an encysted form, figured near by (*b*), they came from the transformation of such a cyst.

As to Dr. Gros' observations about *Gomphonemæ*, they are simply absurd; and the packing of *Naviculæ* (and other forms) into the empty (*Vaucheria* or other alga) filaments is quite a common occurrence. The true explanation of the encysted *Naviculæ* we can easily give, and we have by us at present a slide with over two hundred of these cysts on it. In this case, the Diatom is *Colletonema vulgare*, but we have seen it also with *Synedræ*, *Cocconemæ*, *Gomphonemæ*, and, though more rarely, with mixed forms. To the same category belong figures iii, iv, v, of Smith's *British Diatomaceæ*, vol. ii, pl. C, and very erroneously referred to by him as resulting from the sporangium, figure ii. So also, pl. B, fig. 89, same vol., refered to erroneously as conjugation of *Synedra*. All these, as well as Dr. Bastian's solitary example, are readily explained, and we have repeatedly witnessed the whole phenomenon. It is the work of an Amœba (or an amœboid mass), no way connected with any development, evolution, or transformation. We have an elaborate series of representations, carefully drawn, showing the progress and mode of encysting. Of course, from its very nature, as will be shown, the phenomenon must include that the encysted forms shall be mostly, if not altogether, of one species, and so we find it. Clusters of sessile *Synedræ*, or of stipitate *Gomphonemæ*, or *Colletonemæ*, or small *Naviculæ*, the tubes of the former having become, by quiet, an amorphous jelly; and in such a formless, gelatinous appearing mass, the small *Naviculæ* (*Frustulia* of older authors) are often imbedded. The Amœba, moving freely through the field, over and along the stems of Confervæ, often throwing out long, thread-like arms? of sarcode, like that of Rhizopods, we have repeatedly observed, the moment it reaches a mass of Diatoms (frequently even for one or two), whose bright, clear endochrome showed active life, to spread itself out over them, *completely encysting them*. The Diatoms soon after change appearance; the clear

yellow and olive tints disappear, and only dark red, small masses remain, somewhat like Smith's figure, *British Diatomaceæ*, vol. ii, pl. B, fig. 89. Meanwhile, what is not the least remarkable, a transparent wall, of some tenacity, apparently, forms around the Amœboid mass. After a long period the Amœba escapes by rupturing this outer shell, often at only one point, out of which the mass issues, as a long string, soon gathering itself up, however, to travel on in search of new food. The encysted mass, after the escape of the Amœba, remains, *showing the envelope*, and the frustules are stuck, or half fused, as it were, together. After treatment with acids, &c., in the usual way, for preparing the frustules, this outer envelope disappears, but the frustules still cohere in bunches, as though the silex had been partly dissolved, and they had thus been cemented. We have slides as well as material showing this, in abundance. Sometimes, after thus encysting, the Amœba mass will remain for days, showing no disposition to move away. We think that it will be quite evident that Dr. Bastian's seven encysted *Naviculæ* belonged to the group we have just explained, and are no development of a *Euglena*.

We pass on to figure 85, p. 447, the title of which is, "Origin of Diatoms, Desmids, Pediastræ and Algæ from Euglenæ and other vegetal Matrices," and at p. 444, under the caption, "*Transformation into Diatoms*," it is stated, that "some of them (Euglenæ) are apt, at certain times, to be converted into large Diatoms." The authority for this astounding statement is Dr. Gros, for Dr. Bastian is careful to say, p. 445, "Whilst I have not myself been fortunate enough to trace the actual origin of any of these large Diatoms, I have, on several occasions, been struck with the comparatively sudden appearance of very large specimens (about $\frac{1}{100}'$ in length) of *Navicula librilis* still presenting an embryonic appearance, in vessels containing Euglenæ and Vaucheria." Dr. Bastian apologizes for Dr. Gros' nomenclature (foot note p. 412), but surely he himself was not writing at a period when "precision was not given to nomenclature," "and in a region in which books of reference were not accessible." True, "what's in a name?" yet we fancy that the old names, "hartshorn" and "glue-like," would not suit Dr. Bastian, or modern science, as well as "Ammonic carbonate," or "colloid," aud so it would have been quite as well to give the true name to what is no *Navicula* at all. But this after all is of little moment,

since we know that the Diatom meant is *Cymatopleura solea*, one of the most persistent forms, if indeed there is any difference at all in this respect among the Diatomaceæ. We think that Dr. Bastian cannot consider what he has seen, as to this Diatom, is really of any value in proving transformation of *Euglenæ* into Diatoms. As to the transformations of *Euglenæ* into Desmids, he has been equally unfortunate; but then, "Dr. Gros has observed it on several occasions," indeed!!

Figure 85 *e* (from Gros, of course) shows a *Euglena*, no doubt; it is about the length of (*f*) which is, no doubt, a *Closterium*. Moreover, the *Euglena* is represented with a central transverse blank space, very like that of the *Closterium;* but unfortunately, both ends of a *Closterium*, as everybody knows, are alike, so are not both ends of a *Euglena*. Now, the *Euglena does* undergo many changes, no doubt, but we believe generally, if not invariably, passes from the long spindle shape into the ball. The very slight resemblance as to length and central blank space, is really all that Dr. Gros has to build his transformation upon! Dr. Bastian says, p. 448, "Although I have never seen the final stages of this transformation, I had, even before becoming aware of Dr. Gros' views, noticed the curious fact that very small specimens of Closteria were never to be seen. * * * So that, just as in the case of the large Diatoms already alluded to, their origin by metamorphosis is much more reconcilable with these facts (transformations of Euglena) than with the notion that they are derived from small germs, more especially since no one has ever seen or knows anything about the mode of production of such germs in Closterium." The authority for this latter statement is given—"Pritchard's *Infusoria*, 4th Ed., p. 12." We do not question that such a statement *is* in Pritchard, for there are many erroneous statements in it; it is not on page 12, however; but no doubt this is a misprint, peculiar to American editions. As to the small *Closteria*, considering that Dr. Bastian is not "where books are not easily accessible," we refer him to Ralf's *British Desmidicæ*," T. xxvii; Rev. W. Smith, *A. N. H.*, 1850, p. 4; and Pitchard's *Infusoria* 4th Ed., p. 15. We do not of course vouch for Mr. Jenner's observation, but we think it entitled to as much respect as M. Gros', to say the least; and, if such be the office of the sporangium of *Closterium* (result of conjugation), to serve as a resting spore over the winter, and the final production of broods of young *Closteria*, then

we have, in the very marked difference between the results of conjugation of Desmids and Diatoms, one of the strongest proofs of their complete dissimilarity; for the sporangium of the latter which is generally of much larger size than the parent frustules, serves to restore again the cycle by commencing self-division in a large form, when, by the act long repeated, the frustules had become very small. No doubt many do go on until myriads of small forms appear, thus self-dividing until finally they die out, without any renewal by conjugation.

To finish our remarks on figure 85, (reproduced from M. Gros), (*b*) and (*c*) are referred to as two forms of Diatoms which *may* arise from transformed *Euglenæ*; these are probably *Naviculæ*, one in front, the other in side view; but what *Naviculæ* this representation is too imperfect for decision, (*d*) and (*d'*) are called *Chlamidomonas*, (*sic*) giving origin to Diatoms. Dr. Gros' account of this transformation is too absurd to be worth repeating here. (*d*) Is very much like a small *Amphora* possibly a *Cocconema*, self-dividing, and (*d'*) may be dorsal aspect of a *Cocconema*; any resemblance to *Chlamidomonas*, or any reason to infer development from this is most fanciful. In view, then, of this entirely insufficient evidence, how can Dr. Bastian say, p. 420, that "the actual transformation has been witnessed by independent observers, whereby algoid or Euglenean corpuscles are bodily converted into Diatoms or Desmids"? We have dwelt thus long and particularly on one special portion of the book, because upon this we felt best qualified to act the part of an honest critic. We have no "constitutional objections" or "religious scruples" about accepting anything which can be proved, either as to Archebiosis or Heterogenesis, but we want no fancy pictures. As to other parts of the work, upon which we are not so capable of judging, e. g., such as refer to the development of *Nematoids* from spores of *Vaucheria*, we have no doubt there are many bubbles that might be pricked. This we leave for others, and begging a correction in the Chart, facing p. 552, where it is stated that Diatoms produce *two* embryos, much larger than their parents, which is only partially true, for often there is but one sporangium, and often but one parent frustule, we close, sorry that the author has been led so much astray, and very nearly spoiled a good book.

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THE STRUCTURE OF EUPODISCUS ARGUS.

"On the structure of the valves of *Eupodiscus argus* and *Isthmia enervis*, showing that their silicious deposit conforms to the general plan of deposition in simpler forms," is the title of a paper in the *Monthly Microscopical Journal*, for December, by Henry J. Slack, F. G. S., read before the Royal Microscopical Society, November 6th. In this paper Mr. Slack gives his ideas of the structure of these two diatoms which he formed from their examination with Mr. Wenham's "Improved Reflex Illuminator." Any thing coming from so experienced a microscopist as Mr. Slack is entitled to great consideration, and one would hardly venture to differ from him, without most convincing observations, often repeated. In this case, I believe, that though his figure and description of *Eupodiscus argus* correspond with what he saw, and with what can be seen by other apparatus, yet that he has failed to see or understand the true structure of that species. I shall say nothing of the other diatom at present. I have not used or seen the Reflex Illuminator. From Mr. Wenham's description and the theory of it, it must be a most valuable acquisition to the microscopist, but from my observations on the *Eupodiscus argus*, I cannot think that it can add any thing to our knowledge of its structure; or in other words, that it is unsuitable for that species. Mr. Slack's description of the diatom is: "It is entirely composed of spherules of different sizes and varied aggregation. Radiating from a central portion, occupied by minute and closely packed spherules, bands will be seen proceeding to the circumference, each one composed of minute spherules that appear in close contact under a 1/8, with eye-pieces up to D of Ross' system." Between these bands are larger spherules, frequently but by no means universally arranged in fours. * * * * The figure *Plate xl.* corresponds very well with the description. I have obtained very nearly the same appearance by transmitted light, but such appearance does not disclose the complicated structure. I have long ago made out that to be, that the valve was composed of two layers (of silex), the outer one comparatively opaque or translucent only, with thin apertures in it through which could be seen the "veil" referred to by Mr. Stewart in the discussion of Mr. Slack's paper before the Society. (*See M. M. J.* December, 1872, *p.* 280.)

Very recently I have seen two examples of the shell from which the outer layer is entirely or partially removed. One of them was preserved, and since reading Mr. Slack's paper, has been carefully examined, with the following result: It is made up of radiating "spherules" or granules, but under certain adjustments of focus the radiating arrangement disappears. Figure 1, by my friend Mr. E. Fontarive, shows a portion of this inner plate or veil as seen by Tolles' immersion $\frac{1}{18}$ and B eye-piece, (= about 1400 dia.) where the focus was adjusted to show the "spherules" or bright dots on dark ground; the bright dots not circular but elliptical. Depressing the lens and bringing the *surface* of the valve into focus, (*instead of the cone of light that came through the spherules,*) the appearance changed to Figure 2, when all the radiation disappeared and a cellular appearance was presented, not circular, but of irregular polygonal forms. A little variation of light or focus presents Figure 3. Different observers are likely to put various interpretations on these varying appearances. What is the true one may be an open question. So far, the foregoing relates to the "veil" or inner plate. Mr. Slack suggested the examination with Prof. H. L. Smith's Opaque Illuminator. Acting on that suggestion, I applied it to specimens mounted in balsam with Tolles's $\frac{1}{10}$ in. The effect and result was a new revelation. The diatom now looked like a piece of white lace, with black holes, showing that its "structure" is unlike that of any other of the order known, unless it be its near relative, *E. Rogersii* Bail. Under this mode of examination the diatom is perfectly opaque; under that power ($\frac{1}{10}$ and B) only a very small part of a disc can be illuminated at once; but what is seen, is seen very clearly, and not a sign of a "spherule" can be seen on it, but the surface seems to be composed of an aggregation of shapeless particles. The black holes are usu-

Fig. 1.

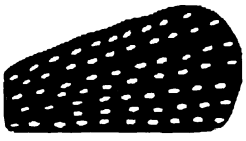


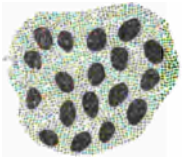
Fig. 2



Fig. 3



Fig. 4.



ally irregularly disposed, but two or three valves have been seen in which there was an approximate radical arrangement. Now while thus illuminated as opaque, the light from the mirror may be turned on, giving the illumination of transmitted light; a wonderful change takes place. Each black hole of the opaque body has become transparent; in each may be seen bright dots, "frequently, but by no means universally arranged in fours," and in the intermediate spaces faintly seen, and consequently looking smaller, other dots or spherules, as in Mr. Slack's figure. Now we have the full explanation of the structure, and the origin, not only of Mr. Slack's figure, but of all the other imperfect figures and descriptions heretofore published.

Fig. 5



Figures 4 and 5 are representations of small portions of the disc as seen with the opaque illuminator, and as represented by two observers. Figure 5, taken by the camera, is a close approximation to the apparent size.

These results were mostly obtained from nine discs mounted by my friend, Samuel Wells, Esq. But I have studied, also, nineteen specimens mounted by Möller. Mr. Wells called my attention to the circumstance that all the *Eupodiscus argus* on the Möller slide were mounted inside up. I examined another slide of Möller's and found the same thing on that, a third slide (*Probe Platte*) was examined by a friend, when the discs proved to be in the same condition. Here were at least forty-one specimens mounted in this manner. It is evident that this must have been intentional, that it is Möller's constant practice for which he may have some special reason, and undoubtedly those studied by Mr. Slack were mounted the same. Now when one of these is examined with the opaque illuminator, the effect is exactly what might be anticipated from the consideration of the structure as above described. The appearance generally is a little different from that, when the outer surface is in view, less sharply marked, but here and there may be seen the minute spherules of the inner plate, *reflecting* the light as glistening balls of glass.

What I have described as the outer opaque surface does not look like a plate; but a crust seems to be the most appropriate term to describe it.

Mr. Slack says that his observations "tend towards the conclusion

that the silicious deposition in diatom probably follows one uniform plan, and that the silex is deposited in spherules, varying in dimensions and degrees of proximity." In the discussion of his paper before the Society he said: "the vegetable matter of the diatom acts chemically upon the silex in the water, which is the condition of colloid silica, and the deposition always take place in the spheroidal form." I have seen abundant evidence that the deposition does not *always* take place in that form. What is the intermediate matter between the "spherules of different degrees of proximity?" The very species under consideration furnishes evidence of another mode of deposition. The "feet" or processes of this diatom are sometimes quite conspicuous, and are as structureless, smooth and transparent as glass; not a trace of spherules can be detected in them with the highest powers applied. The chemical action of the vegetable matter must be the same in one part of the valve as another.

Mr. Slack also says: "Until Mr. Wenham's researches settled the old disputes as to whether the markings on ordinary diatoms were elevations or depressions," certain phraseology (quoted by Mr. Slack) "might be admissible, but careful examination of the objects with the best optical means now at the command of the microscopist may be expected to banish such terms as "areolar," "cellules," &c., from the descriptions of diatoms." In the discussion following the reading of the paper, "Mr. Brooke would like to know how it happened, if the structure of these objects were really hollows and not bosses, that the line of fracture runs between the dots and not through them. He had fractured a great many diatoms under the microscope, and had never seen a single instance in which the line of fracture did not run equi-distant between two consecutive dots."

Unfortunately for me I have never seen Mr. Wenham's "settlement" of that "dispute," and consequently I cannot be influenced by it. Since reading the report of that discussion, I have examined *some hundreds* of broken diatoms, in which the fracture runs exactly as Mr. Brooke never saw it. In this state of the observations, as Mr. Slack does not specify what was meant by "ordinary diatoms," I shall continue to use the terms "cellules" and "areolæ," until *convincing evidence* is offered that they are improper, for either ordinary or extraordinary diatoms.

Charles Stodder.

Boston.

THE FLORA OF CHICAGO AND VICINITY.

V.

JUNCACEÆ.

LUZULA, DC. *L. campestris*, DC.; Glencoe and Hinsdale; common.

JUNCUS, L. *J. Balticus*, Dethard; lake shore; common. *J. bufonius*, L.; rare (*H. A. W.*). *J. nodosus*, L.; Hyde Park and S.; common.

PONTEDERIACEÆ.

PONTEDERIA, L. *P. cordata*, L.; Evanston and Miller's; common.

COMMELYNACEÆ.

COMMELYNA, Dill. *C. Virginica*, L.; sandy hills S. of Miller's; abundant.

TRADESCANTIA, L. *T. Virginica*, L.; N., W., and S.; very abundant.

XYRIDACEÆ.

XYRIS, L. *X. flexuosa*, Muhl., Chapm.; S. of Calumet; rare.

CYPERACEÆ.

CYPERUS, L. *C. diandrus*, Torr.; abundant, especially S. *C. erythrorhizos*, Muhl.; along I. C. R. R., between Oakland and Hyde Park; common. *C. inflexus*, Muhl.; low, sandy soil N. and S.; common. *C. Michauxianus*, Schultes.; with *C. erythrorhizos*; not common. *C. Schweinitzii*, Torr.; sandy soil near lake shore and at Miller's; abundant. *C. filiculmis*, Vahl.; abundant S. and S. E.

DULICHIUM, Richard. *D. spathaceum*, *Pers.*; borders of sandy sloughs at Miller's; abundant.

ELEOCHARIS, R. Br. *E. obtusa*, Schultes; Hyde Park and S.; common. *E. acicularis*, R. Br.; Hinsdale; common.

SCIRPUS, L. *S. validus*, Vahl.; bogs; common. *S. debilis*, Pursh; Hyde Park; common.

ERIOPHORUM, L. *E. Virginicum*, L.; sloughs near Miller's; abundant. *E. polystachyon*, L.; low prairies W. and S.; common.

RHYNCHOSPORA, Vahl. *R. glomerata*, Vahl.; Miller's; not abundant.

SCIRPUS, L. *S. lacustris*, Mich.: Calumet and Miller's: common.

*CAREX, L. *C. aperta*, Boott., var. *B.*: woods at Riverside; abundant. *C. aurea*, Nutt.: very abundant about sloughs at Pine Station, where also is found the var. *androgyna*, Olney. *C. Buxbaumii*, Wahl.; bogs: S. of Hyde Park and at Miller's: common. *C. cephalophora*, Muhl.: Hinsdale: common. *C. crawfordii*, Boott.; Pine Station and Gibson's: not common. *C. Crawei*, Dew.: S. W. of Hyde Park; rare. *C. filiformis*, L.: sloughs at Pine station; not common. *C. Grayii*, Carey; Thatcher and Riverside, near banks of Desplaines R.; abundant at latter locality. *C. hystricina*, Willd.; Pine station: abundant. *C. intumescens*, Rudge: S. of Michigan City: common. *C. lanuginosa*, Michx.: Glencoe: common. *C. laxiflora*, Lam.; Glencoe: abundant: var. *blanda*, Boott; Downer's Grove: very abundant. *C. Muhlenbergii*, Schk.: lake shore, near Kenwood; not common. *C. paniculata*, L.: var. *Bebbii*, Olney, and var. *Meadii*, Olney; Hinsdale: common: var *tetanica*, Olney; S. W. of Hyde Park: not common. *C. Pennsylvanica*, Lam.; woods at Hinsdale and Calumet: common. *C. pubescens*, Muhl.; Hinsdale and Downer's Grove; common. *C. rosea*, Schk.; Hinsdale; not common. *C. scoparia*, Schk.; Hyde Park; not common. *C. sparganioides*, Muhl.: lake shore, near Kenwood; also at Hinsdale; common. *C. sterilis*, Willd.; Hyde Park: not common. *C. stricta*, Lam. var.; Hyde Park; common. *C. tentaculata*, Muhl.; Michigan City; abundant. *C. vulpinoidea*, Michx.; Hinsdale; common.

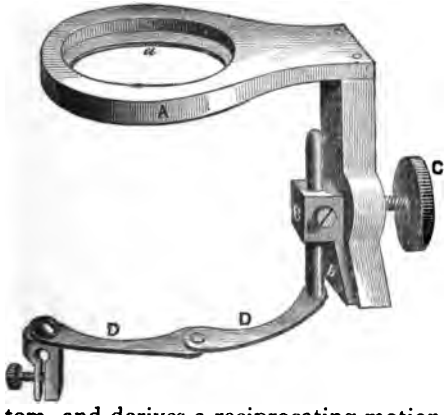
H. H. Babcock.

Chicago.

*From determinations kindly furnished by Col. S. T. Olney, of Providence, R. I.

A NEW MECHANICAL FINGER.

HAVING occasion to mount a gathering of diatomaceæ, mixed with a very large proportion of fine sand, I endeavored to pick out some of the larger specimens with a fine needle, attached to the end of an inch objective with beeswax, and succeeded in making some satisfactory mounts in this primitive fashion, but the obvious inconveniences of such an arrangement led me to seek for a better apparatus. I could only learn of two mechanical fingers previously designed, one by Professor Smith, and the other by Zentmayer, and after a trial of the former, kindly loaned by Mr. Stodder, and an examination of the drawing and description of the latter, I had one made different from either.



It is represented in the accompanying drawing, in which A is the frame which slips over the nose of the tube of the microscope, and is held in place by the objective, which, when screwed up, presses against the thin flange *a*, projecting inwards.

The socket B is soldered to a flat spring, *b*, fastened by two screws at the bottom, and derives a reciprocating motion from the milled head screw C. The two arms, D D, swing round the end of the objective, sustaining the needle-holder on the further side. In using this finger, a slide containing the mixed sand and diatomaceæ is placed upon the stage and moved about by the ordinary movements of a mechanical stage. When the desired object is brought into the centre of the field, a turn of the screw C brings the end of the hair or needle into the field and down upon the object, a backward turn of the course adjustment clears the stage, and the glass cover on which the specimen is to be mounted can then be substituted for the slide containing the mixture. I have used this finger for some time, and its performance is very satisfactory; its advantages lie in its cheapness

and simplicity, and in its keeping the hair or needle out of the way and out of the field of view until wanted, when a turn of the screw C brings it into position.

It was made from my drawing, with great nicety, by Messrs. Buff & Berger, of this city, and one like it can be made by any philosophical instrument maker; but any microscopist ordering one should have it fitted to his instrument so that it will stay in place on the nose without the assistance of an objective.

Samuel Wells.

Boston, Nov. 4, 1872.

THE YELLOWS OF THE PEACH.

On the first of July last I commenced a series of experiments by the moist process with the bark of a peach tree affected with the yellows. Into five glass receivers I placed, respectively, a few drops of water, just sufficient to form a moist atmosphere in each. Into the first I put a piece of bark affected with the yellows; into the second a piece of bark from a healthy peach tree; into the third a handful of peach leaves from the unhealthy tree; into the fourth a similar quantity from the healthy tree; and into the last portions of bark from the healthy and unhealthy trees mentioned. All the specimens were secured from outward atmosphere. The temperature of the room in which the specimens were kept was frequently at 90° Fahrenheit. These conditions were highly favorable to the development of such fungi-germs as mature under excess of heat and moisture. Previous to arranging the specimens in the receivers they were examined minutely with a low power, but no signs of fungi were visible. On the fifteenth day the unhealthy specimens in Nos. one and five exhibited on their external surface a spotted appearance. When viewed with a power of 75 diameters they were seen to consist mostly of the translucent, yellowish-brown, spiral, thread-like fungus known as *Nemaspora.*

When a small portion of this fungus is placed under a one inch object glass and secured in the usual manner by means of a disc, with dilute gum-water, the spiral forms are seen to dissolve gradually, and ultimately to form a yellowish stain. On viewing it with

an eighth, it appears to be a mass of curved spores, resembling in form caraway seeds, but invisible to the naked eye. Each spore has a life-like motion confined to the centre of its own. When they are treated to the action of nitric, muriatic, and nitro-muriatic acids, no immediate change is observable; and in those strong acids the life-like motion continues, which, I think, proves that the motions are not the result of any form of organic life, but simply what is known as Brownian motion, which is frequently seen when minute particles of inorganic matter are placed under a high power. When the spores are combined either with concentrated sulphuric acid or caustic potash they become completely destroyed, forming a homogeneous mass, and their organic structure is no longer visible.

About the twentieth day mycelium was found in abundance growing from the spiral threads, resembling double-celled *Puccinia*, the spores varying in number from one to ten, and so small that a power of one-eighth was required to give good definition. Since contact with water dissolves *Nemaspora* without destroying the life of the spores, it is evident that the action of rain or washes of pure water will only tend to diffuse the spores over the body of the tree and roots, while the application of solutions of sulphuric acid and alkalies will destroy them. Hence a remedy may be found for peach-yellows in the application of alkalies and sulphates, and their compounds, to the bark and roots of the trees. Statements have frequently been made that the application of hot lye has been known to cure peach-yellows when applied to the bark and roots. My own observations seem to confirm these common rumors.

In the fifth receiver, the healthy bark was not contaminated, seemingly, with the *Nemaspora*, notwithstanding its immediate contact during several weeks with the unhealthy bark. As might be expected, the common molds, *Penicillium* and *Mucor*, grew all over the surface of the specimens, healthy and unhealthy. The leaves in Nos. three and four were next examined. They had been subjected to the same treatment as the bark. The healthy leaves, although confined during four weeks in a moist atmosphere, at a temperature ranging from 80° to 90°, exhibited no signs of mildew. A split branch to which the leaves were attached exhibited a small portion of *Mucor* fruit, and mycelium on the sap-wood and pith; but the unhealthy leaves were completely covered in two weeks with mycelium, and the fruit of the common blue, yellow, and black *Penicil-*

lium and *Mucor.* I have repeated these experiments several times, always with the same results. It is evident that the healthy leaves possess an antiseptic substance, which prevents the growth of common molds on them. A portion of healthy and unhealthy leaves from the trees above mentioned was analyzed in the laboratory to determine the respective amounts of moisture, organic matter and ash in them, and gave the following results:

Healthy peach leaves:		Unhealthy leaves:	
Moisture,	29.20	Moisture,	36.9
Organic Matter.	63.22	Organic Matter,	59.4
Ash,	7.58	Ash,	3.7
	100.00		100.0

The fact of the absence of ash or solid matter and of the increase of moisture in the unhealthy leaves, would of itself account for their greater tendency to mold. Since leaves do not absorb earthy matter from the atmosphere, it is evident that the cellular structure of the tree has in some way failed to perform its functions; for, had the ascending sap carried with it potash, lime, or other earthy matter, the leaves would have been stored with them, since the leaves have no power to evaporate them. The deficiency of earthy matter in the leaves may also account for the absence of ash in the fruit. If the theory is well founded that the leaves elaborate juice for the growth of the fruit, the leaves being deprived of proper nourishment, the fruit cannot mature. It has been long observed that trees affected with the yellows, fruit earlier and mature prematurely, and soon decay. The presence of a larger amount of sap in the unhealthy than in the healthy, indicates an earlier and greater flow than in that of the healthy tree. The presence of watery sap in the leaves, twigs, and buds would induce naturally an early growth of fruit and premature decay. From these and other observations the disease seems traceable to the body of the tree or roots. Applications of washes in this case to the leaves would probably prove useless, but if applied to the bark and roots, might prove curative; and for that purpose, judging from microscopic observations, I would recommend the frequent application of hot lye as the best substance.

Thomas Taylor,
Microscopist to the Department of Agriculture, Washington, D. C.

MICROSCOPIC APPEARANCES OF CANCER CELLS.

Twenty years ago, the opinion was pretty generally entertained in the medical world, that the microscope, if used by skillful hands, was entirely competent to decide as to the malignancy or benignancy of any and all morbid growths. In the twenty-fifth volume of the *American Journal of the Medical Sciences*, may be found a lengthy and elaborate article, evidently the product of much and careful study, in which the author, Dr. F. Donaldson, of Baltimore, sets forth the distinctive characteristics of cancer cells, and adds many figures intended to illustrate these peculiarities. Dr. Donaldson's attempts at classifying cancer cells should have convinced him, it would seem, that these cells at least, construe very liberally their obligations to conform to the requirements of a uniform law of type. He makes six varieties:—The polygonal, or more or less spherical and ovoid cell; the caudated cell; the fusiform cell; the concentric cell; the compound, or mother cell; and agglomerated nuclei, connected by granular homogenous substance.

This classification must certainly have been conceived in a truly liberal spirit. It is broad-gauged enough to cover every cell in every part and organ of the body, whether in health or in disease; and not only that, but also to include every cell in every plant and animal that flourishes upon or walks over the earth, and of every inhabitant of the boundless ocean. Let any one set himself squarely down to the work of imagining what the microscopic appearance of "the polygonal, or more or less spherical and ovoid cell" must be, and he will find himself engaged in a task which is limited only by his powers of imagination. Indeed I can scarcely conceive of an exercise better calculated to develop that faculty of the mind. The fact is, Dr. Donaldson's observations, instead of proving that anything like uniformity exists in the structure and appearance of cancer cells, very clearly prove that the distinguishing churacteristic of these bodies is a most perverse and constant want of uniformity, and in doing this, he made a very important and welcome addition to our knowedge of the minute structure of cancerous growths. The very active researches which have been going on in every department of pathological histology during the last few years, have but served to buttress and substantiate the statement that *there is no typ-*

ical cancer cell, as there is a typical pus cell and a typical epithelial cell. The very nature of cancer; its comparatively rapid growth; its lawless invasion and destruction of all adjacent tissues; its short life and imperfect organization, and, moreover, its rapid decay, are, each and all, sufficient reasons for expecting many and varying types of cell growth. These several peculiarities are worth glancing at a little more closely.

First. Cancer is a structure of essentially feeble organizing power; in other words, it is endowed with a low grade of vital power. In its mode of growth its behavior is strikingly like the so-called *fungus* or *proud-flesh,* which is so apt to sprout up in wounds which do not heal kindly. Indeed, it is not perhaps far out of the way to describe it as a sort of fungus growth implanted within or engrafted upon normal tissue. Hence, like all productions of like nature, its growth is rapid as compared with healthy structures; and this very rapidity necessitates incompleteness, or a faulty and unfinished growth. Hence, also, we should expect to find cells in all stages of growth, as well as a multiplicity of cell forms. We find caudate cells, pushing out their processes where they meet with the least resistance—in other words, growing wherever they find room to grow, without, in their haste, stopping to consider the results; we find half-grown cells, looking like overgrown nuclei; large, awkward, angular cells, frequently with several nuclei, (the so-called "mother" or "brood" cells) which seem only intent upon leaving a numerous progeny behind them; cells with one or more "buds" or processes protruding from their walls, which is also a form of cell mutilplication; and, finally, cells which combine two or more of these various phases of growth. Therefore, one peculiarity of cancer cells depends upon their rapidity of, as well as faulty growth.

Secondly. Cancer is notoriously lawless and unsparing in its progress. Unlike other morbid growths, it invades all structures which occupy its pathway, infiltrating glands, insinuating its growing germinal matter between the fibres of muscle, or actually trespassing upon the substance of the individuul fibre itself; plowing its way into and through osseous tissue; perforating membranes; tapping blood vessels; surrounding and compressing nerves; and, ultimately, destroying and disintegrating whatever it touches. But in accomplishing this work of destruction, cancer liberates very many

formed and forming cells which do not properly belong to it. That is, the *normal* cells of *healthy* structure, whether it be glandular or otherwise, become admixed with the *abnormal* cells of diseased (cancerous) structure, just so fast as the latter invades and disintegrates the former. A specimen of epithelial cancer of the lip before me, shows a multitude of epithelial cells which cannot be distinguished from those of the healthy structure, and these are most abundant at or near the junction of the healthy with the diseased tissue. A specimen of encephaloid of the liver contains very many cells precisely like, and undoubtedly identical with, the seecreting cells of this organ, and in both instances, the cells of health and disease lie in the most intimate relations. Here, then, we have another reason for the great variety in regard of form and size, which we so constantly meet with in the study of cancer cells.

Thirdly. The term of life of any given cancer cell is necessarily a brief one; its organization necessarily imperfect, or of low type, and its decay and disorganization rapid. Hence, in almost every specimen of cancer cells, we may expect to find, and, indeed *do* find, many cells in different stages of decay, and, consequently, undergoing various morphological changes. That part of the cell which is first formed is likewise first to die, and the external part, or periphery, (the so-called "formed material") is now almost universally regarded as the oldest portion of the cell; hence we find cells with angular or roughened boundaries, because the external portion is slowly or rapidly disintegrating; but sometimes large masses of the "formed material" will disappear at once, and this gives to many cells their awkward and ill-shaped appearance; others seem to wither and become shriveled, or, possibly, dessicated; hence, we find cells presenting a pinched, dried, starved appearance: others still maintain their integrity in regard of form and size, but become filled with granular matter, which is probably the product of fatty degeneration; hence, we find cells which are dark or granular, or cloudy or nebulous in appearance. Again, all cancers speedily undergo—or are constantly undergoing—fatty degeneration, both within and between their component cells; or perhaps it is more correct to say that this change always commences *in* the cells, but that the fat drops are shortly liberated as the result of cell-decay. Here then we find still another element of complexity—namely, the presence of many fat globules, both floating free between the pro-

per cells, and likewise enclosed within, or forming a part of, these cells.

It need not, therefore, be a matter of surprise that cancer cells should bid defiance to any and all rules of classification. It is, indeed, rather a matter of surprise that any attempt at such a classification should ever have been made, seeing that, for all diagnostic purposes, we are far better off as we are; and, for all pathological purposes, we should not be a hair's breadth nearer the truth regarding the real *nature* of cancer *with* a formal classification than we are *without* one.

A very important practical question, however, still presents itself, namely, can cancer be diagnosticated with any degree of certainty, by its microscopic appearances? It is easy enough to answer this question to my own satisfaction; it is possibly less easy to make the response intelligible to those unaccustomed to the study of cell forms. I believe that the practical microscopist can have little difficulty in satisfying himself as to the character of a morbid growth, provided always he receives the specimen in a state fit for examination. I have myself received many specimens for examination which consisted of little bits, snipped from the surface of a morbid growth, and then rolled in a dry paper or rag, and carried in the pocket for hours afterward, until they became perfectly hardened and dry. The idea of forming a conclusion from such specimens as these is simply absurd and ridiculous. The constituent cells of such a specimen must of very necessity undergo changes of form and consistency which cannot be obviated by any artificial means; hence, any conclusions drawn from them must be, to say the least, very unreliable. A specimen intended for microscopic examination should be taken from beneath the surface of the tumor, and as near its centre as possible; it should be examined at once whenever this is practicable, and in any event, it should be kept moist until it passes to the hands of the microscopist. If these conditions be complied with, I believe that a correct conclusion can be arrived at in so large a proportion of cases, that the exceptions are not worth considering, if the examination be made by one accustomed to the study of morbid growths. Morover, I believe that any one who possesses a microscope of fair quality, and who can use it with a reasonable amount of common sense, can satisfy himseif as to whether a morbid growth is or is not cancerous in its nature. Possibly a

description of cancer cells will still be expected. They are at once very easy and very difficult to describe. If the illustrations and descriptions of these cells given by Glüge, Paget, Henry H. Smith, Beale, Gross, Moore, Bennett and others, be consulted, they will be found to be all very *like* and all very *unlike*, except when the same engravings have been used by two or more different authors; and this is simply because no two illustrations representing *actual specimens* can be alike. The distinguishing characteristic of cancer cells is a *want of uniformity*, or an absolute non-conformity to any law of type—and this for reasons which I have already given. It is, indeed, true that a cancerous growth, especially in its early stages, attempts to produce cells which are more or less like those of the tissue out of which, or within which, or near which, it is differentiated, as we so often see in the case of epithelial cancer; but this attempt is never successful, even at the very beginning, and it is shortly abandoned in so far as an allegiance to typical forms is concerned. Hence this very want of uniformity in regard of cell forms, constitutes the pivot upon which the diagnosis turns when the microscope is appealed to. It is not because we *can* classify cancer cells that we *call* them such; but rather because we *cannot* classify them. It is not because they are old microscopic acquaintances that we call them malignant; but rather because they are always strangers. In fact it is just precisely this diversity of appearance that proves their malignancy; for it indicates their rapid and lawless growth, their disrespect for and destruction of adjacent tissues, and their rapid death; and these, taken together, constitute the very essence of malignancy. If fifty specimens of keloid be examined, the ultimate elements of each one will be found to be, not precisely alike—for cells are no more precisely alike than leaves or apples or men are precisely alike—but formed on the same general model as regards shape and size, and, therefore, conforming to a law of type. If fifty specimens of ordinary fatty tumor be examined, the same law will be found to hold good. Moreover, in both these cases, the morbid growth will show some respect for the rights of neighboring tissues. If, indeed, the latter are crowded out of their rightful position, they will not therefore be denied the privilege of existing somewhere else. But on examination of twice this number of cancer specimens will only the more absolutely demonstrate the fact that in place of law of type we have lawlessness of type, and that this very

lawlessness is the microscopic peculiarity of cancer. Hence, in the so-called "innocent" or "benignant" growths, we are to look for a kind of multiform unity, and in malignant growths, we may, with equal confidence, expect a uniform multiformity, or an absolute non-conformity to any ideal cell type.

One other, and scarcely less important point should be noticed. Errors of *location* are generally coincident with diversity of form in cancerous growths, and this should be taken into account in establishing the diagnosis. The very fact that a mass of cells have infiltrated or invaded a tissue or organ, and that they persistently and unaccountably vary in size, form and behavior from the normal cells of such tissue or organ, is all but, and perhaps quite, sufficient to stamp them with the seal of malignancy. They are at once recognized as marauders and pirates, intent only upon the destruction of everything within their reach. To attempt to describe a specimen of cancer cells, is to huddle together as many epithets as we can lay our hands on, to pester and perplex the dictionary makers by bringing forth a new progeny of words, and appropriating them to the same use, and, after all, to fail of giving a reliable description of bodies which obstinately refuse to look twice alike. It seems to me that we best describe cancer cells when we simply say that we *cannot* describe them; when we say that they present an endless diversity of forms, and that, *therefore*, they are cancer cells.

In conclusion, I would lay down the following simple rules for drawing the distinction between innocent and morbid growths: whenever a description of *one* of the cells of a microscopic specimen is a description of *all* of its cells, the chances are as ten to one that it is *not* cancer;—whenever, on the other hand, the cells of such a specimen are so varied in form and size that philology and ingenuity and imagination, and the most unflincing resolution combined, utterly fail to accomplish the task of describing them, the chances are as ten to one that the specimen is from a malignant growth, whatever may be its name or location.

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THE INFLUENCE OF LIGHT UPON LIFE.

Lavoisier somewhere says: "Organization, voluntary movement, life, exist only at the surface of the earth, in places exposed to light. One might say that the fable of Prometheus' torch was the expression of a philosophic truth that the ancients had not overlooked. Without light, Nature was without life; she was inanimate and dead. A benevolent God, bringing light, diffused over the earth's surface organization, feeling, and thought." These words are essentially true. All organic activity was very clearly at first borrowed from the sun, and if the earth has since stored away and made its own a quantity of energy, that sometimes suffices to produce of itself that which originally proceeded from solar stimulus, it must not be forgotten that those living forces, of startling and complex aspects, sometimes our pitiless enemies, often our docile servants, have descended, and are still descending upon our planet, from the inexhaustible sun. The study of animal life shows us by striking instances the physiological efficacy of light, and the immaterial chain, it may be called, which links existence with the fiery and abounding heart of the known universe.

In plants, respiration at night is the reverse of that by day. There are infusoria which behave, under the influence of light, exactly like the green portions of plants. These microscopic animalcula are developed in fine weather in stagnant water, and in breathing produce oxygen at the expense of the carbonic acid contained in the liquid. Morren saw that the oxygenation of the water occasioned by these little beings varied very perceptibly in the course of twenty-four hours. It is at the minimum at sunrise, and reaches its maximum toward four in the afternoon. If the sky is overcast, or the animalcula disappear, the phenomenon is suspended. This is only an exception. Animals breathe at night in the same way as in the daytime, only less energetically. Day and night they burn carbon within their tissues, and form carbonic acid, only the activity of the phenomenon is much greater in light than in darkness.

Light quickens vital movements in animals, especially the act of nutrition, and darkness checks them. This fact, long known and applied in practical agriculture, is expressly noted by Columella. He recommends the process of fattening fowls by rearing them in small dark cages. The laborer, to fatten his cattle, shuts them up

in stables lighted by small low windows. In the half-light of these prisons the work of disassimilation goes on slowly, and the nutritive substances, instead of being consumed in the circulating fluid, more readily accumulate in the organs. In the same way, for the sake of developing enormous fat livers in geese, they are put into dark cellars, kept entirely quiet, and crammed with meal.

Animals waste away as plants do. The absence of light sometimes makes them lose vigor, sometimes entirely changes them, and modifies their organization in the way least favorable to the full exercise of their vital powers. Those that live in caverns are like plants growing in cellars. In certain underground lakes of Lower Carniola we find very singular reptiles resembling salamanders, called proteans. They are nearly white, and have only the rudiments of eyes. If exposed to light they seem to suffer, and their skin takes a color. It is very likely that these beings have not always lived in the darkness to which they are now confined, and that the prolonged absence of light has destroyed the color of their skins and their visual organs. Beings thus deprived of day are exposed to all the weaknesses and ill effects of chlorosis and impoverishment of the blood. They grow puffy, like the colorless mushroom, unconscious of the healthy contact of luminous radiance.

William Edwards, to whom science owes so many researches into the action of natural agents, studied, about 1820, the influence exercised by light on the development of animals. He placed frogs' eggs in two vessels filled with water, one of which was transparent, and the other made impermeable to light by a covering of black paper. The eggs exposed to light developed regularly; those in the dark vessel yielded nothing but rudiments of embryos. Then he put tadpoles in large vessels, some transparent, others shielded from the light. The tadpoles that were shone upon soon underwent the change into the adult form, while the others either continued in the tadpole condition or passed into the state of perfect frogs with great difficulty. Thirty years later, Moleschott performed some hundreds of experiments in examining how light modifies the quantity of carbonic acid thrown off in respiration. Operating on frogs, he found that the volume of gas exhaled by daylight exceeds by one-fourth the volume thrown off in darkness. He established, in a general way, that the production of carbonic acid increases in proportion to the intensity of light. Thus, with an intensity represented by

3.27, he obtained 1 of carbonic acid, and, with an intensity of 7.38, he obtained 1.18. The same physiologist thinks that in batrachians the intensity of light is communicated partly by the skin, partly by the eyes.

Jules Béclard made more thorough researches. Common flies' eggs, taken from the same group, and placed at the same time under differently-colored glasses, all produce worms. But if the worms, hatched under the different glasses, are compared at the end of four or five days, preceptible differences may be seen among them. Those most developed correspond with the violet and blue ray; those hatched under the green ray are far less advanced; while the red, yellow, and white rays exert an intermediate action. A long series of experiments on birds satisfied Béclard that the quantity of carbonic acid thrown out in breathing, during a given time, is not sensibly modified by the different colors of the glasses the animals are placed under. It is the same with small mammifers, such as mice; but it is to be observed in this case that the skin is covered either with hair or feathers, and the light does not strike the surface. The same physiologist examined also the influence of the different-colored rays of the spectrum on frogs. Under the green ray, the same weight of frogs produces in the same period of time a greater quantity of carbonic acid than under the red ray. The difference may be a half greater; it is usually a third or a fourth greater; but if the skin is afterward taken off the frogs, and they are replaced under the same conditions, the result alters. The amount of carbonic acid thrown out by the flayed frogs is greater in red than in green light. A few experiments tried by Béclard on the exhalation of the vapor of water by the skin show that in the dark, temperature and weight being alike, frogs lose by evaporation a half or a third less moisture than under white light. In the violet ray the quantity of moisture lost by the animal is perceptibly the same as in white light.

Light acts directly on the iris of almost all animals, and thus produces contraction of the pupil, while heat produces the reverse phenomena. This stimulus is observed in eyes that have been separated for some time from the body, as Brown-Séquard has shown.

Bert lately took up some very curious experiments on the preference of animals for differently-colored rays. He took some of those almost microscopic crustacea, common enough in our fresh

waters, the daphne-fleas, remarkable for their eager way of hurrying toward light. A number of these insects were put into a glass vessel, well darkened, and a spectrum of the ray then thrown into it. The daphnes were dispersed about the dark vessel. As soon as the spectrum-colors appeared, they began to move, and gathered in the course of the luminous track, but when a screen was interposed they scattered again. At first all the colors of the spectrum attracted them, but it was soon noticed that they hurried much more toward the yellow and green, and even moved away a little if these rays were quickly replaced by the violet. In the yellow, green, and orange parts of the spectrum there was a thronging and remarkable attraction. A pretty large number of these little beings were remarked in the red, too, a certain number in the blue, and some, fewer in proportion to the distance, in the most refrangible portions of the violet and ultra-violet. For these insects, as for ourselves, the most luminous part of the spectrum was also the most agreeable. They behaved in it as a man would do who, if he wished to read in a spectrum thrown about him, would approach the yellow and avoid the violet. This proves, in the first place, that these insects see all the luminous rays that we see ourselves. Do they perceive the chlorific and chemic rays, that is to say, the ultra-red and ultra-violet ones, which do not effect our retina? Bert's experiments enable us to answer that they do not. That physiologist is even led to assert that, with regard to light and the different rays, all animals experience the same impressions that man does.

Let us now look at the influence of light upon the color of the skin in animals, noticing first the being which presents the strangest peculiarities in this respect, the chameleon. This animal, indeed, experiences very frequent modifications of color in the course of the same day. From Aristotle, who attributed these changes to a swelling of the skin, and Theophrastus, who assigned fear as their cause, to Wallisniéri, who supposes them to result from the movement of humors toward the surface of the animal's body, the most different opinions have been expressed on this subject. Milne-Edwards, thirty years ago, explained them by the successive inequalities in the proportions of the two substances, one yellowish and the other violet, which color the skin of the reptile, inequalities due to the changes in volume of the very flattened cells that contain these substances. Bruck, renewing these researches, proves

that the chameleon's colors follow from the manifold dispersion of solar light in the colored cells, that is to say, from the production of the same phenomenon remarked in soap-bubbles and all very thin plates. Its colors, then, come from the play of sunlight among the yellow and violet substances distributed very curiously under its wrinkled skin. It passes from orange to yellow, from green to blue, through a series of wavering and rainbow-like shades, determined by the state of the light's radiation. Darkness blanches it, twilight gives it the most delicate marbled tints, the sun turns it dark. A part of the skin bruised or rubbed remains black, without growing white in the dark. Bruck satisfied himself, moreover, that temperature does not effect these phenomena.

All animals having fur or feathers are darker and more highly colored on the back than on the belly, and their colors are more intense in summer than in winter. Night-butterflies never have the vivid tints of those that fly by day, and among the latter those of spring have clearer, brighter shades than the autumn ones. The gold-and-azure dust that adorns them harmonizes with the tones of colors in surrounding nature. Night-birds, in the same way, have dark plumage, and the downiness of their coverings contrasts with the stiffness of those that fly by day. Shells secluded under rocks wear pale shades, compared with those that drink in the light. We have spoken above of cave-animals. What a distinction between those of cold regions and those of equatorial countries! The coloring of birds, mammals, and reptiles, peopling the vast forests or dwelling on the banks of the great rivers in the torrid zone, is dazzling in its splendor. At the north we find gray tints, dead and of little variety, usually close upon white, by reason of the almost constant reflection from snow.

Not only the color of organized beings, but their shape too, is linked with the action of light, or rather of climate. The flora of the globe gains increasing perfection as we go from the poles toward the equator. The nearer these beings approach the highest degree of heat and light, the more lavishly are richness, splendor and beauty bestowed on them. The energy and glory of life, perfect forms as well as brilliant arraying, are the distinguishing mark of the various and manifold races of tropical regions, giving this privileged world its characteristic aspect. A pure emanation from the sun, Nature here lives wild and splendid, gazing unshrinkingly, like the Alpine

eagle, on the eternal and sublime source which inundates it with heat and glow. Look, now, at the regions of the pole! A few dwarfish shrubs, a few stunted and herbaceous plants, compose all its flora. Its animals have a pale covering and downy feathers; its insects, sombre tints. All around them are the utmost limits of life —ice invades everything, the sea alone still breeds a few acalephs, some zoöphytes, and other low rudimentary organizations. The sun comes aslant and seldom. At the equator he darts his fires, and gives himself without stint to the happy Eden of his predilection.

Revue des Deux Mondes.

(Translated for the Popular Science Monthly.)

EDITOR'S TABLE.

THE NEW BRITISH SCIENTIFIC EXPEDITION.—The *Challenger*, an eighteen-gun screw corvette, commanded by Capt. Nares, with Commander Maclear, of the Eclipse Expedition, as second, left Sheerness in December for a lengthy voyage which is expected to confer very great benefits upon the scientific world.

The vessel is fitted out for the purpose of sounding, dredging and investigating the science of the deep sea, the Government having been very liberal in providing all the funds and necessaries required. The scientific staff consists of Professor Wyville Thomson, as director; Dr. Von Willmoes Saum, Mr. H. Moseley, and Mr. J. Murray, as naturalists; Mr. Buchanan as chemist; with sundry other competent assistants in the different branches of science. The Challenger is well supplied with boats of different kinds, including a steam pinnace, and carries an ample assortment of the various appliances used in dredging, and an almost inexhaustible stock of spirits and bottles for the preservation of objects. Under the experienced supervision of Professor Wyville Thomson, she has been furnished with everything that can possibly aid in exploring the different seas through which the vessel will pass; and a small aquarium will afford an opportunity for studying interesting animals alive.

The first haul is expected to be made in the Bay of Gibraltar, after which she will probably visit Madeira, sailing thence for the West Indies, and after touching at sundry places on the Brazilian coast, cross to the Cape of Good Hope. The Islands in the Southern Ocean will then be examined, with a run to the ice, and after exploring the Australian seas and Oceania, the expedition will proceed to Japan, Kamschatka, passing through Behring's Straights to the Northern Ice, and back to Vancouver's Island and the western coast of the Americas, round the Horn and home. The voyage will occupy probably four years, and the expedition cannot fail to be of lasting benefit to science, if not directly to the nation at large.

INOCULATION WITH DEAD BLOOD.—It is well known that surgeons are often seriously injured by accidentally cutting themselves with instruments that have been recently used for dissecting purposes. The wounded part swells, and mortification often ensues, necessitating amputation and sometimes causing death. In order to determine the poisonous properties of this putrid blood, M. Davaine communicates to *Les Mondes* the result of several experiments made upon rabbits. The liquid used was the blood of an ox that had been ten days slaughtered. This, by subcutaneous injection, he administered to his subjects in varying quantities, obtaining by successive dilutions with water the most infinitesimal attenuations. Killing one animal, he would take its infected blood and force the same into the veins of another, and so on until he reached what he terms the twenty-fifth generation. On this last experiment he says: "Four rabbits received respectively one trillionth, one ten-trillionth, one hundred-trillionth, and one quadrillionth of a drop of blood from a rabbit belonging to the preceding generation that had died from the effects of a one-trillionth dose. Of the four, but one animal died—that which received the one ten-trillionth. It appears then, that the limit of transmissibility of the poison in the rabbit reaches the one-trillionth part of a drop of decayed (*septique*) blood."

"POPULAR SCIENCE."—Science will never be successfully popularized until the popularizer be himself deeply read in Science. No smatterer need expect to inculcate a love for any exact science by cramming for special occasions, and presenting crude and ill-digested material, thereby incurring the risk of perpetrating the most egregious blunders.

A lecturer on Natural History, in discoursing on entomological topics, recently made the following statements. They are no more astounding than many others made at the same time, which some regard for the credulity of our readers compels us to omit:

That no true insects are wingless. That the *Hymenoptera* are so called because their *wings are split* nearly up to the body. That in America we have no locusts. That on the western plains certain chocolate-colored beetles are so numerous and provided with antennæ *so rigid* that *with them* they are able *to goad* the buffaloes to death. That the house *spider* has 3,000 eyes. That the different sizes of flies are due entirely to difference of species.

The lecture was appropriately illustrated by means of charts on which were represented creatures to worship many of which would involve no violation of the second article in the Decalogue. *Platysamia Cecropia* and *Danaus Archippus*, figured on the same chart, were of about the same size, the Cecropia stilted high up on the tips of his *four* legs, having possibly just emerged from the cocoon which hung *by one end* from the twig on which the *Cecropia* was supposed to be crawling.

And what shall be said of "this larvæ" and "that larvæ," of "these probosces" and "those maxilla," of "his eggs" and "his ovipositor."

Why will our dear friends at the East still persist in coming to these "Western Wilds" to instruct us? Such doses as these are grievously unpalatable. Shall

the representative educational men of one entire Western State sit in silence at the feet of any peripatetic philosopher because he comes from the neighborhood of the Modern Athens, and accept without remonstrance *such* science? Shades of *Linnæus*, *Fabricius*, *Godart*, forbid!

FISH CULTURE IN MICHIGAN.—Mr. N. W. Clark, of Clarkston, Mich., is largely engaged in hatching whitefish and salmon *ova* for the *United States Fish Commission*. He has arranged with James W. Milner, Esq., Deputy Commissioner, to hatch from fifty to one hundred thousand salmon *ova* which have been procured from the Fish Commissioner of Maine, Mr. Atkins. A considerable portion of the young fish is to be sent to California for experiment in her waters.

A few gentlemen have erected a hatching house at Clarkston, of sufficient capacity to hatch half a million ova, and caused to be placed in its troughs that number of whitefish ova. They were placed there November 15, 1871, and about fifty per cent. of them were duly hatched on the 1st of April, 1872, and distributed in the waters of the Detroit river, and a few of the inland lakes in Oakland county. Again on the 13th day of November, 1872, these same gentlemen caused to be placed in the same hatching establishment 500,000 more of the whitefish ova, and these are now doing finely, and it is expected that a much larger per cent. of the young fry will be ready for distribution in April, 1873, as the capacity of the hatching house has been doubled for the accommodation of the General Government.

PHOTOGRAPHIC SPECTRAL LINES.—We call our readers' attention, lest they may have overlooked the fact in admiring the resolution of the object itself, to the remarkable series of spectral lines lying entirely outside the frustule, in the Woodburytype illustration of Dr. Woodward's resolution of *Frustulia Saxonica* in our last number.

GUNDLACH'S OBJECTIVES.—We are glad to be able to note (see advertisement in this number) the removal of Mr. Gundlach from Berlin to the United States. He has established himself at Hackensack, N. J., where he will devote himself to the production of first-class objectives only. His scale of prices will be found to be very low. In our next number we shall publish a paper by Prof. F. Ardissonne, of Milan, communicated to the *Nuevo Giornale Botanico Italiano*, giving the relative resolving power upon well known test-objects of various objectives of the best continental makers, including Mr. Gundlach, from which it appears that Mr. G's objectives did him high honor. One of them, a sixteenth, equivalent to his No. VII. (European nomenclature), now belongs to Mr. H. H. Babcock, of Chicago, who mentions it to us in terms of unqualified praise.

IRIDESCENT ENGRAVINGS.—The beautiful iridiscence of the pearl is shown by the microscope to be due to the presence upon its surface of exceedingly fine ridges or lines, the edges of which unequally refract the rays of light and produce many shades of color of marvelous delicacy. This effect may be artificially produced upon glass and other substances, by cutting lines thereon of sufficient

fineness. Mr. L. M. Rutherford, the well-known scientist of New York, was one of the first to construct a machine capable of engraving these iridescent lines, which he ruled upon glass, as test objects, the cutter being worked by an electro-magnetic machine. M. Nobert, however, has surpassed as yet all others in this field by the delicacy of the rulings of his famous Test Plate. Another of these instruments is now to be seen at Harvard University, and is thus described in the Boston *Globe:*

"Among the many curious inventions existing beneath the dome of the Cambridge observatory, there is a machine which is used to delineate upon glass the figure of a circle or square, by means of finely drawn lines. This machine, which is the invention of Mr. Rogers, who is connected with the observatory, is very simple in its operation, and draws each line with an accuracy which is very surprising. This is done by means of a graduated plate of metal, which acts upon a very sharply and very finely pointed needle, so that it may be set at any distance from a line already drawn. By actual trial, the skillful inventor drew upon a small piece of glass twenty-four lines, separated by a distance of one 2400th of an inch, in about a minute's time. These wonderful lines could be easily counted through a microscope, but viewed with the naked eye they formed a single, but somewhat imperceptible, line. It was nearly impossible to imagine the exceedingly minute distance which separated them. Parallel lines had been drawn upon other plates of glass, which, though apparently single, were found, when placed beneath the microscope, each to be composed of several distinct lines. A circle, also, which was about a quarter of an inch in diameter, contained sixteen hundred lines. The light was very beautifully reflected from their minute sides, and the circle glistened and sparkled with all the colors of the rainbow. Upon one plate of glass had been traced circles within circles, the lines of which they were composed being scarcely discernible; but when perceived through the miscrocope each line assumed a perfect precision, and the delicate symmetry of each circle came out in exquisite relief. The extremely minute space in which this immense number of circular lines was contained appeared to be very wonderful. There were many other drawings, which were equally astonishing, and which showed equally well the fine skill of Mr. Rogers."

THE INCREASE OF DIATOMACEÆ BY SELF-DIVISION.—In a late communication from Prof. Smith relative to the self-division of diatoms, he says: "It may be objected that if by self-division the frustules become smaller, then the persistent filamentous forms, at least some of them, should, upon measurement, actually exhibit this gradation in size. I reply that this is the case, and in a filament of thirty-seven double frustules of a large *Melosira Moniloformis*, I find the middle frustules larger by .0001″ (with the 1/8″ objective 30 divisions of my Powell & Lealand thread micrometer), and so repeatedly of other chains of frustules. It would at first appear that the largest frustules should be at the ends, and not the middle of a filament. We must remember, however, that although the two larger primary valves may be carried to the ends if the filament remains unbroken, yet all the time self-division is occurring between; so that a series of nodes, or swellings, will exist all along the chain. For example, if after the

formation of, say, half a dozen frustules, so nearly the same size that we may consider them equal, we now suppose self-division to occur simultaneously, so that each frustule produces six others, then these latter, smaller than the older ones, would be distributed throughout the chain, and these again, all simultaneously dividing, would give rise to still smaller ones interposed; and it is manifest that a chain, if parted, would very likely be severed at the smaller frustules, and the partial filaments would have the larger and older (perhaps thus more siliceous) frustules, near the middle, unless we should chance to find one of the ends with the valve of the primary frustule, which would rarely happen. As for W. Smith's broods of young frustules, these are but casts of *amœba*, excrementitious. I have seen them hundreds of times, and quite often a heterogeneous mass of small *Cymbelleæ*, *Gomphonemeæ* and *Naviculeæ*. All this will be fully illustrated in the proper place."

DEPTH OF SOIL.—Dwellers in the West, on prairies, or on the alluvial bottoms of the South, have naturally a sincere contempt for regions where the depth of soil is measured by inches, rather than feet. Nevertheless, in the most highly cultivated parts of Europe the average depth of soil is but about six inches. Experiments made in Germany show that if a soil six inches deep is represented by fifty, a soil seven inches deep would be represented by fifty-four, and one only three inches deep would be represented by thirty-eight. In New England the soil is probably from four to six inches deep, some of the rich alluvial meadows being deeper, and some of the arable uplands much less. Any soil can be deepened by proper cultivation, and every one knows that the deeper it is the more luxuriant will be the crops. The cost of cultivating a three-inch soil is not very much less than that of a six-inch soil, and the crop raised on the latter is nearly certain to be double that of the former, or, if we take the German rule, as fifty to thirty-eight. Enthusiastic microscopists say that they have found that the roots of red clover will, in some instances, penetrate to the depth of six feet, and those of winter wheat to a depth of seven feet. If planted in a deep hole filled with rich loam, parsnips and the like will sometimes send their rootlets to the bottom. All these instances are merely to show what vegetables will do if they have a chance, but it is a fair inference that the deeper a soil is, the deeper it will naturally become. A great mass of roots decaying every year constantly increases the amount of vegetable matter in the soil, makes it better able to resist the effects of drought, gradually converts the upper part of underlying strata into something better, and thus tends to increase its own productive powers.

THE LOST ARTS.—Mr. Wendell Phillips recently delivered, in New York, for the how-many-hundredth time? his famous lecture on this subject, and the *Tribune* reported it in full. It is, of course, interesting; but it strangely confounds legend and fact; and ignores some plain and elementary distinctions. For instance, Mr. Phillips takes pains to argue from the minuteness of ancient gem-carvings that the microscope is not so modern as we think. Does he not know the radical difference between the magnifying glass and the microscope? From an alleged ring with a gem in it, through which Nero looked at the gladia-

tors, he infers that Nero had an opera-glass. This repeats the error we have pointed out. The knowledge of the magnifying glass among the ancients is not at all surprising, and need not be so ingeniously inferred. There is a description of a burning-glass in the *Nubes* of Aristophanes; any drop of water could have given the idea; and it is scarcely possible to manufacture glass without obtaining portions of it in forms that will magnify objects. But the invention of the microscope is a very different affair, and came to pass only after the discovery of the *camera obscura*. The story of the way in which Solomon's temple (or some other ancient edifice) was protected with spear-heads, which the sentinels touched, to ascertain the electrical condition of affairs, if correctly reported, seems to indicate that the ancients knew as much about lightning-rods as Mr. Phillips. The fact is, that the "lost arts" are very few, and mostly not worth the finding. Outside of the glass business, which is the strongest point in their favor, the achievements of the ancients were principally accomplished by individual patience and manual dexterity on the one hand, or by masses of men under despotic direction, on the other. An element in both cases was the small value of labor. The art which they had, and we have comparatively lost, is the art of wasting time.

THE NINETEENTH BAND AND TOLLES' EIGHTEENTH.—Prof. H. L. Smith has lately been in Boston, and writes as follows, concerning a resolution of this famous test: The angular aperture, as stated by Mr. Tolles was 130°, as I had forgotten the number of lines in this band, my attempts at a "count," were certainly unbiased; estimating the centre of a band, and counting outwards, in three several counts the difference was but one or two from the true number. Subsequently with a 1-6th which, as immersion, had an angle (as stated by Mr. Tolles) of 170°, I saw *Amphipleura pellucida* handsomely resolved. The illumination was by lamp (in day time) at an incidence of 45°. The specimens were, as I understood Mr. Tolles, received from Dr. Woodward. The resolution was very clear and distinct.

MAN AS THE INTERPRETER OF NATURE.—The recent address of Dr. Carpenter to the British Association is extremely suggestive. Speaking of man as the interpreter of Nature, Dr. Carpenter insists on the necessity of inquiring into the origin and validity of human conceptions, and especially of such dominant ideas as matter and force, cause and effect, law and order. The ancients imposed their own conceptions upon nature as eternal laws, and this error is as active and misleading to-day as it ever was. We may, if we choose, believe in the uniformity of nature, but this, and other beliefs which serve as a foundation of scientific reasoning must be inquired into, and not absolutely assumed. It is to be noticed, for instance, that, although modern science is held to have shown that the sun is invested with a chromosphere of incandescent hydrogen, this assertion is but an induction from observed phenomena, and depends for its validity upon the unproved assumption that a certain line seen in the spectrum of a hydrogen flame means hydrogen when seen in the spectrum of the sun's chromospere. Dr. Carpenter then passes to the controversy between the intuitionalists

and the sensationalists, and would reconcile the two by the idea that the intuitions of one generation are the embodied experiences of the previous generation. Knowledge cannot descend from father to son, but an increased aptitude for acquiring knowledge may be inherited, and so it happens that conceptions that prove inadmissible to the minds of one generation subsequently find acceptance and are acknowledged to be self-evident. In regard to those who insist that force does not exist, and that we know nothing but matter and its laws, Dr. Carpenter holds that it would be more plausible to regard matter as an intellectual conception and force, as precisely the one thing of which we have a direct knowledge, as we have personal experience of it in resistance and weight, derived from our own perception of exertion. That interpretation of nature which does not co-ordinate the idea of force with that of motion, and regard the former as the cause of the latter, must be very inadequate. The doctor, in fine, believes that modern science has been too exclusively phenomenal—too much confined to generalizations.

FOOD FISHES.—The report of the Fish Commissioners of Maine, to be made to the legislature this month, contains a very full discussion of the question of restoring to the rivers the different food fishes with which they formerly abounded. They argue that in view of the increased and constantly increasing price of living, the products of the water should be increased, and food that is now confined to the tables of the rich, should be afforded to the poor at a cheap price. They present the opinion of some of our most eminent naturalists that something of the old-time experience can be regained when the rivers of New England were almost blockaded by shad, salmon and alewives seeking to ascend for the purpose of depositing their spawn; and they argue that the decrease of these species is the cause of the diminution of the cod and other deep-sea species near the coast. The people living along the rivers have at last come to demand the restoration of the river fish, and are disposed to aid the Commissioners in enforcing the laws for the construction of proper fishways, and for preventing the criminal destruction of fish. The experiments of hatching ova by artificial means have been attended with great success; and the Commissioners ask for a small appropriation for engineering services, and for obtaining a good clear highway from the upper waters of the rivers to the ocean, by suitable fishways through dams, and keeping the streams clear by prohibiting the throwing of saw-dust, edgings, &c., into them, and they ask the co-operation of mill-owners in their efforts.

FREY ON THE MICROSCOPE.*—This is an admirable work, elegantly translated. We hardly know whether to praise most its high scientific character, its condensed, though clear and beautiful style, or its thoroughly practical treatment of the subject.

The work begins with a sketch of the theory of the microscope and a description of different styles of instruments, more especially the continental forms.

*THE MICROSCOPE AND MICROSCOPICAL TECHNOLOGY; A text-book for Physicians and Students, by Dr. Heinrich Frey, Professor of Medicine in Zurich. Translated from the German and edited by George R. Cutter, M. D., Clinical Assistant to the New York Eye and Ear Infirmary. New York: William Wood & Co.

Then follows a chapter on drawing, measuring, and micro-photography. Then a chapter on testing the glasses, and test objects; with the names of the more prominent objective makers and the character of their glasses. Then come hints in regard to the use of the instrument, and several chapters on the preparation of objects, the use of chemical agents, cutting sections, injecting, and mounting. This portion of the work occupies two hundred and twenty-eight pages. The remainder of the work is devoted to normal and pathological histology, and the methods of study of each particular organ. Finally, an appendix is added, containing a price list of different microscope makers, a valuable feature.

The work can hardly take the place of Beale's work, but it is one which every histologist will find very useful.

STRICKER'S HISTOLOGY.*—The want of a comprehensive work on Histology has long been felt, and it is the object of the present work to supply it. It is made up of various articles written by different persons. The opening article on the preparation of tissues, by Stricker, is valuable but incomplete. The second article, also by Stricker, on the cell, evidently cost the writer a great deal of trouble. It is a careful summary of all that is known on the subject. It is however, diffuse and unsatisfactory. The writer seems unable to come to a definite conclusion on any point. He gives facts and theories, qualifies them with doubts, and then checks the doubts with counter-doubts. He states his own opinions so mildly that they are likely to be overlooked among the mass of doubts and conflicting opinions by which surrounded. The remaining articles on less pretending subjects are good, though most of them are diffuse and dry. The work as a whole, is cumbrous. It would be improved by more illustrations. It is well translated, however, and is the best work on the subject in the language.

RINDFLEISCH ON PATHOLOGICAL HISTOLOGY.†—The seeker after knowledge (knowing of Prof. Rindfleisch's reputation in Germany as a good writer as well as a standard authority in pathology) who opens this translation for the first time expecting to find a clear exposition of the subjects on which the work treats, will be sorely disappointed. He will be met by an array of German idioms and bad English frightful to contemplate, especially if he has before him the task of mastering its six hundred and eighty-one muddy pages. These blemishes, however, appear to be mainly the fault of the translator, for the matter of the work is good, the arrangement fine, and the different articles are exhaustive, but short and to the point.

The author is cautious to a marked degree in his reception of theories which are

*A MANUAL OF HISTOLOGY, by Prof. S. Stricker, of Vienna, in co-operation with Th. Meynert, F. Von Recklenghausen, Max Schultze, W. Waldeyer, and others. Translated by Henry Power of London; James J. Putnam, and J. Orne Green, of Boston; Henry C. Eno, Thos. E. Satterthwaite, Edward C. Seguin, Lucius D. Bulkley, Edward L. Keyes, and Francis E. Delafield, of New York. American translation edited by Albert H. Buck, Assistant Aural Surgeon to the New York Eye and Ear Infirmary. New York, William Wood & Co.

†A TEXT BOOK OF PATHOLOGICAL HISTOLOGY.—An introduction to the study of Pathological Anatomy, by Edward Rindfleisch, O.ö., Professor of Pathological Anatomy in Bonn. Translated from the second German edition, with permission of the author, by William C. Kloman, M. D., assisted by F. T. Miles, M. D., Professor of Anatomy, University of Maryland. Philadelphia, Lindsay & Blakiston.

not well proven, and he seems to ride no hobbies. He accepts cautiously *Conheim's* theory of the origin of pus by the "emigration" of the colorless blood-corpuscles, modifying it by the statement that it has by no means been proved that there are not other sources for its formation.

He rejects their clinical history as a basis for the classification of tumors, and classifies them according to their microscopic structure. He occupies a neutral ground in the dispute in regard to the constitutional or local origin of malignant growths. The section devoted to the diseases of the lungs is a very satisfactory one. The author accepts the theory of the non-tubercular origin of most cases of *phthisis* which Niemeyer so ably advocates, but cautions his readers against restricting too much the domain of tuberculosis, thinking that tubercles have more to do with many cases of lung disease than the advocates of the catarrhal origin of *phthisis* are accustomed to acknowledge. The work as a whole, is excellent, and one which no one who wishes to be fully informed of the present state of pathology can afford to be without.

WHY CAMPHOR SPINS ABOUT IN WATER.—In a late number of the *Popular Science Monthly*, Prof. Clifford says: "If small pieces of camphor are dropped into water, they will begin to spin round and swim about in a most marvellous way. Mr. Tomlinson gave, I believe, the explanation of this. We must observe, to begin with, that every liquid has a skin which holds it; you can see that to be true in the case of a drop, which looks as if it were held in a bag. But the tension of this skin is greater in some liquids than in others; and it is greater in camphor and water than in pure water. When the camphor is dropped into water, it begins to dissolve and gets surrounded with camphor-and-water instead of water. If the fragments of camphor were exactly symmetrical, nothing more would happen; the tension would be greater in its immediate neighborhood, but no motion would follow. The camphor, however, is irregular in shape; it dissolves more on one side than the other; and consequently gets pulled about, because the tension of the skin is greater where the camphor is most dissolved. Now, it is probable that this is not nearly so satisfactory an explanation to you as it was to me when I was first told of it; and for this reason: By that time I was already perfectly familiar with the notion of a skin upon the surface of liquids, and I had been taught by means of it to work out problems in capillarity. The explanation was therefore a description of the unknown phenomenon which I did not know how to deal with as made up of known phenomena which I did know how to deal with. But to many of you possibly the liquid skin may seem quite as strange and unaccountable as the motion of camphor on water."

FORESTS AND FRUIT-GROWING.—In the same journal we find the following: Fruit has become a necessary of life—a great variety of fruit indeed, and a great deal of it; and this will become more and more the case with the increase of intelligence and thrift. The great abundance of most kinds of fruit for the last two or three years may cause us to feel a security, which is not well grounded, with regard to the conditions of climate necessary to the unfailing production of fruit. Only within a few years past have there been seasons when the fruit-crop

was very light, and not at all adequate to the demand. One of the causes of this is the capriciousness of the seasons, and this capriciousness, I believe, is becoming constantly greater as the country grows older.

An inquiry, then, of much scientific interest, and of great material importance, has reference to what may be the cause of this increasing uncertainty of the fruit-crop. In the early settlement of the country, it was easy to grow peaches, even in localities where growing peaches now seldom gladden the eye. In Ohio between the parallels of 40° and 41°, for example, peach-buds were seldom injured by winter or spring frosts, and the crop was abundant almost every year when the country was "new." For the last twenty-five years, peaches miss oftener than they hit, and in many parts this has told so fearfully against the enterprise of production that scarcely a peach tree is now to be seen.

The clearing of the country had made this change. The continued clearing of the country will increase the mischief still more. The growing of peaches and of most other fruits will be driven, as indeed it already has been, to special localities and special soils. It is now for such localities to look out in time and preserve as far as possible the favorable conditions they now have, and if possible to increase them."

GREAT FIRES AND RAIN-STORMS.—In an article published in the *Journal of the Franklin Institute*, July, 1872, by Prof. I. A. Lapham, assistant to the Chief-Signal officer U. S. A., entitled, *The Great Fires of 1871 in the Northwest*, we find the following in regard to the burning of Chicago: "During all this time—twenty-four hours of continuous conflagration upon the largest scale—no rain was seen to fall, nor did any rain fall until four o'clock the next morning; and this was not a very considerable 'down-pour,' but only a gentle rain, that extended over a large district of country, differing in no respect from the usual rains. The quantity, as reported by meteorological observers at various points, was only a few hundredths of an inch. It was not until four days afterward that any thing like a heavy rain occurred. It is therefore quite certain that this case cannot be referred to as an example of the production of rain by a great fire. Must we therefore conclude," says Prof. Lapham, "that fires do not produce rain, and that Prof. Espy was mistaken in his theory on that subject? By consulting his reports (Fourth Report, 1857, p. 29), it will be found that he only claimed that fires would produce rain under favorable circumstances of high dew-point, and a calm atmosphere. Both of these important conditions were wanting at Chicago, where the air was almost entirely destitute of moisture, and the wind was blowing a gale. To produce rain, the air must ascend until it becomes cool enough to condense the moisture, which then falls in the form of rain. But here the heated air could not ascend very far, being forced off in nearly an horizontal direction by the great power of the wind. The case therefore neither confirms nor disproves the Espian theory, and we may still believe the well-authenticated cases where, under favorable circumstances of very moist air and absence of wind, rain has been produced by large fires." Prof. Lapham also remarks, "The telegraph-wires indicated no unusual disturbance of the electric condition of the atmosphere." Upon reading this last remark, the question occurs to us, Can there not

be a change in the electrical state of the atmosphere which, although too small to manifest itself upon telegraph-wires, may occasion storms?

THE VELOCITY OF NERVE CURRENTS.—An interesting article on this subject, in the *Revue des deux Mondes*, by R. Radau, has been translated by Mr. A. R. Macdonough, and is published in the January number of the *Popular Science Monthly*. It is shown that thought never springs instantaneously under the influence of an external cause. The nervous current, which transmits sensations to the brain, requires a certain appreciable time; and a similar interval is consumed in the transmission of the commands of the will to the members, which obey the motive thought. Several attempts have been made to measure this velocity. A doctor of the middle ages, conceiving the nerves to carry a material fluid, fancied its speed must bear a relation to that of the blood, in the inverse ratio of the areas of their respective channels. This calculation gave "six hundred thousand million yards per minute—six hundred times the rapidity of the motion of light." [We quote this from the article above mentioned. Whether the mediæval doctor or his modern commentator is to blame, we do not say; but the rate given is not six hundred times—it is only about thirty times—the velocity of light.]

Haller, reasoning from the number of letters he could pronounce in a minute, and the number of muscular motions requisite for each, deduced a speed of 154 feet per second for the nervous current. His reasoning was erroneous and his data were loose; but he stumbled upon a tolerable approximation of the true result.

Helmholtz, the distinguished German physicist, has solved the problem by a new and satisfactory method. He measured directly, by means of a galvanic chronoscope, the period that elapsed between the irritation of a frog's muscle and its contraction; also the period between the excitation of an adherent nerve, and the contraction of the muscle. The difference shows how long a time is required to send the news of the irritation to the brain, and the command of the brain to the muscle. The speed of the nervous current was thus found to be, in the frog, nearly eighty feet per second. Improvements of the experiment, and its application to the human subject, have shown that sensation is transmitted in the human body between ninety and one hundred feet per second. This is certainly not very high speed; and it shows pretty clearly that the "nerve-fluid" is not identical with electricity, though electrical currents exist in nerves. The reader will see, on reflection, that people killed by lightning must die without pain, since the electrical discharge traverses the body so much more swiftly than the currents of sensation.

SPONTANEOUS MOVEMENTS IN PLANTS.—In the *Popular Science Review*, Mr. Alfred W. Bennett presents an extremely interesting account of the spontaneous motions and irritability observed in the vegetable world, of which the sensitive plant (*Mimosa*) affords a well-known, but by no means a solitary, example. One of the commonest and most mysterious of such phenomena is that of the convolution of climbing plants. These, as is notorious, always twine round their support in one direction, that is, always from right to left or from left to right, and always,

for the same species, in the same direction. This is manifested when there is no support, or when the end of the growing shoot stands or hangs free from the prop to which the lower portion already clings. When a climbing plant first springs from the ground, the extremity of the shoot performs slow gyrations in the air, as if, as Darwin expresses it, it were *searching* for a support. This movement, Mr. Bennett says, is spontaneous; that is, it is not the necessary result of known physical laws acting upon the individual. If it were so, individuals of different species, under similar conditions, would turn in the same direction; and individuals of the same species, under different conditions, would follow different directions. Mr. Bennett does not distinctly claim for plants the actual possession of a voluntary or sentient faculty; but he points out that facts do not support the dogma of a clear line of demarcation separating the animal from the vegetable kingdom—the power of voluntary motion belonging to one and not to the other.

THE DIFFERENCE BETWEEN THE TWO SIDES OF THE HEART.—In an article entitled *Foul Air and Disease of the Heart*, by Dr. Cornelius Black, in the December number of the *Popular Science Monthly*, the following statement is made as to the difference between the two sides of the heart:

"Why are the affections of the two sides of the heart essentially different in their nature? Why do those of the left side of the heart point to an inflammatory origin; those of the right side of the heart, with but few exceptions, to a non-inflammatory origin? There must be some cause for this difference. What is it? The reason is found in the difference which exists between the constitution of the blood which reaches the left side of the heart from the lungs, and that which reaches the right side of the heart from the general system. The blood reaching the left side of the heart from the lungs has been replenished with all the elements necessary for the growth of the tissues; it has been purified, renovated, and vivified by its oxygenation in the lungs, and it is thus rendered in the highest degree stimulating to the left heart. The blood reaching the right side of the heart from the general system has been deprived, by the requirements of growth, of the chief portion of its nutrient materials; it has been fouled by the *débris* of tissue-waste; it has been further poisoned by its impregnation with carbonic-acid gas: it is therefore a depressant, rather than a healthy excitant, to the right heart. True, it brings with it to the chambers of the right heart the products of the digestion of food; but what are they, either as nutrients or excitants, when they reach that point? They are no more than inert, unusable, passive elements. Not until they have passed to the lungs, and have there received the vivifying influence of oxygen, can they enter into the real composition of the blood, and thus become active, exciting, disposable constituents of it."

THE BLOOD CIRCULATION AND HEART DISEASE.—In the same paper, the necessity of a uniform and normal circulation of the blood is thus urged:

"The third great vital function which influences the degenerative tendency of the heart is that of the circulation of the blood. To preserve the health of the tissues, the blood must not only be pure and rich in the materials of growth, but it must flow with a certain speed through all the blood-vessels. If the speed

with which the blood moves is on the side of either *plus* or *minus* of the standard of health, disease will shortly arise. If it is on the side of *plus*, active disease of the heart, where that organ is the one to suffer, will follow. If on the side of *minus*, tissue degeneration will ensue. Active disease will be the consequence before middle age; degeneration after that period.

"These facts teach that all violent and long-continued efforts of the body should be avoided. They hurry the heart's action to an inordinate degree; they cause it to throw the blood with great force into the extreme vessels, and, as there is almost always one organ of the body weaker than the others, the vessels of this organ become distended, and, remaining distended, the organ itself becomes diseased. Running, rowing, lifting, jumping, wrestling, severe horse-exercise, cricket, football, are fruitful causes of heart-disease. Those which require the breath to be suspended during their accomplishment are more fruitful causes in this respect than those which require no such suspension of the breathing. Rowing, lifting heavy weights, wrestling, and jumping, do this; and, of these, rowing is the most powerful for evil. At every effort made with the hands and feet, the muscles are strained to their utmost; the chest is violently fixed; no air is admitted into the lungs; blood is thrown by the goaded heart with great force into the pulmonary vessels; they become distended; they at length cannot find space for more blood; the onward current is now driven back upon the right heart; its cavities and the blood-vessels of its walls become in like manner distended; the foundation of disease is laid. Hypertrophy, hæmoptysis, inflammatory affections of the heart and lungs are the consequences in the young; valvular incompetency, rupture of the valves or of the muscular fibres of the heart, pulmonary apoplexy, and cerebral hæmorrhage, are too frequently the immediate consequences in those of more mature years."

CARBOLIC ACID IN SMALL-POX.—In a recent number of the *Lancet*, Dr. Alexander Watson recorded several cases of small-pox and scarlet fever, in which the external application of carbolic acid met with marked success. In the case of one patient with small-pox, whom he saw at the period when *papulæ* appeared, he ordered an enema, and then had the patient—a girl of eleven years—sponged all over with carbolic acid soap-suds. On the next day, a severe attack of confluent small-pox was threatened, but the child was sponged as she had previously been, and then her whole body was painted with the carbolic acid glycerine of the British Pharmacopœia. Five grains of Dover's powder were then given to allay irritability, and the little girl slept quietly for several hours, when she was sponged again. No vesicles formed and the patient was convalescent in a few days. Carbolic acid was, in the meantime, plentifully used about the room.

THE MACROPODE.—This little fish forms the subject of a paper communicated to the French Academy of Sciences by M. N. Joly. Eight years ago, M. Agassiz said that he had found among the fish tribe metamorphoses as considerable as those which had been remarked in reptiles; and this is a case in point. The egg

of the macropode, not bigger than a poppy-seed, when hatched is perfectly transparent and lighter than water. It is hatched in about sixty-five hours, just as is the case with the egg of the tench. But on account of this rapid birth, the creature is necessarily in an imperfect state. It makes its appearance in the shape of a tadpole, the head and trunk of which are attached to a large belly, the tail being free and surrounded with a natatory membrane which is exceedingly transparent. Although the animal seems to have no striped muscular fibers, it is very nimble under the microscope and is not more than a millimeter and a half in length. Its head has two large eyes still deprived of their pigment; there is no mouth, and no digestive apparatus either. But the heart is already active, and some circulation is perceptible in the upper part of the tail. There are no gills, so that respiration must be effected through the skin. There are no secretory organs and no fins. The same as in all fish, the nervous system is formed at an early period, and is composed of two parallel cords which branch out into the head. Of the skeleton, nothing appears as yet but the dorsal cord. Numerous pigmentary spots appear all over the body. A short time after, the mouth, intestines, liver and air bladder are formed, together with the gills. New vessels gradually make their appearance, while the earlier ones are obliterated. The caudal natatory membrane is gradually formed into two pectoral fins, and brilliant scales cover the body, and from that moment the creature assumes the shape of a regular fish. Here, therefore, we have changes similar to those which are observed in Planer's lamprey, in insects and in crustacea. This is an important fact, since naturalists had hitherto denied the existence of such changes in fish.

INFLUENCE OF VARIOUSLY COLORED LIGHT ON GROWTH.—This subject is at present attracting a good deal of attention, and, strange to say, it is regarded by many as a new matter for investigation, a patent even having been recently granted for the use of blue glass in the cultivation of plants. Several years ago, a committee of the British Association for the Advancement of Science investigated the whole question very thoroughly, and at various times individual observers have devoted their attention to the subject. The general result seems to be that growing plants thrive best in white light, while seeds, during the process of germination, do best under blue rays. The well-known seedman, Charles Lawson, of Edinburgh, thus details the results of some experiments made by him in 1853: "I had a case made, the sides of which were formed of glass, colored blue or indigo, which case I attached to a small gas stove for engendering heat; in the case shelves were fixed inside, on which were placed small pots wherein the seeds to be tested were sown. The results were all that could be looked for; the seeds freely germinated in from two to five days only, instead of from eight to fourteen days as before. I have not carried our experiments beyond the germination of seeds, so that I cannot afford practical information as to the effect of other rays on the after culture of the plants.

I have, however, made some trials with the yellow ray in preventing the germination of seeds, which have been successful; I have always found the violet ray prejudicial to the growth of plants after germination."

BEWARE OF GREEN WALL PAPERS.—A physician in Western Massachusetts recently had a lady patient who, for several weeks, had been suffering from nausea, general prostration, and other symptoms of slow poisoning. Failing to discover the cause of the symptoms, says the *Hartford Courant*, as a last resort the doctor requested her to move from her chamber, the walls of which were covered with paper of a very light shade of green, so light, indeed, that in the evening it could scarcely be distinguished from white. After leaving the room the symptoms immediately disappeared, and the patient rapidly recovered. A sample of the paper was forwarded for analysis to the State chemist, Mr. Joseph Hall, and was found to contain a large quantity of arsenic. Mr. Hall obtained the poison in the various forms of metallic arsenic, yellow tersulphite, silver arsenite and arsenious acid or common white arsenic. He estimates that every square foot of this innocent-looking paper contained an amount of the poison equivalent to five grains of arsenious acid, or double the fatal dose for an adult person. This, in the moist, warm weather of last July and August, was amply sufficient to keep the air of a room constantly impregnated with the poison, and any person occupying such a room would be as certainly poisoned as though the arsenic had been taken into the stomach.

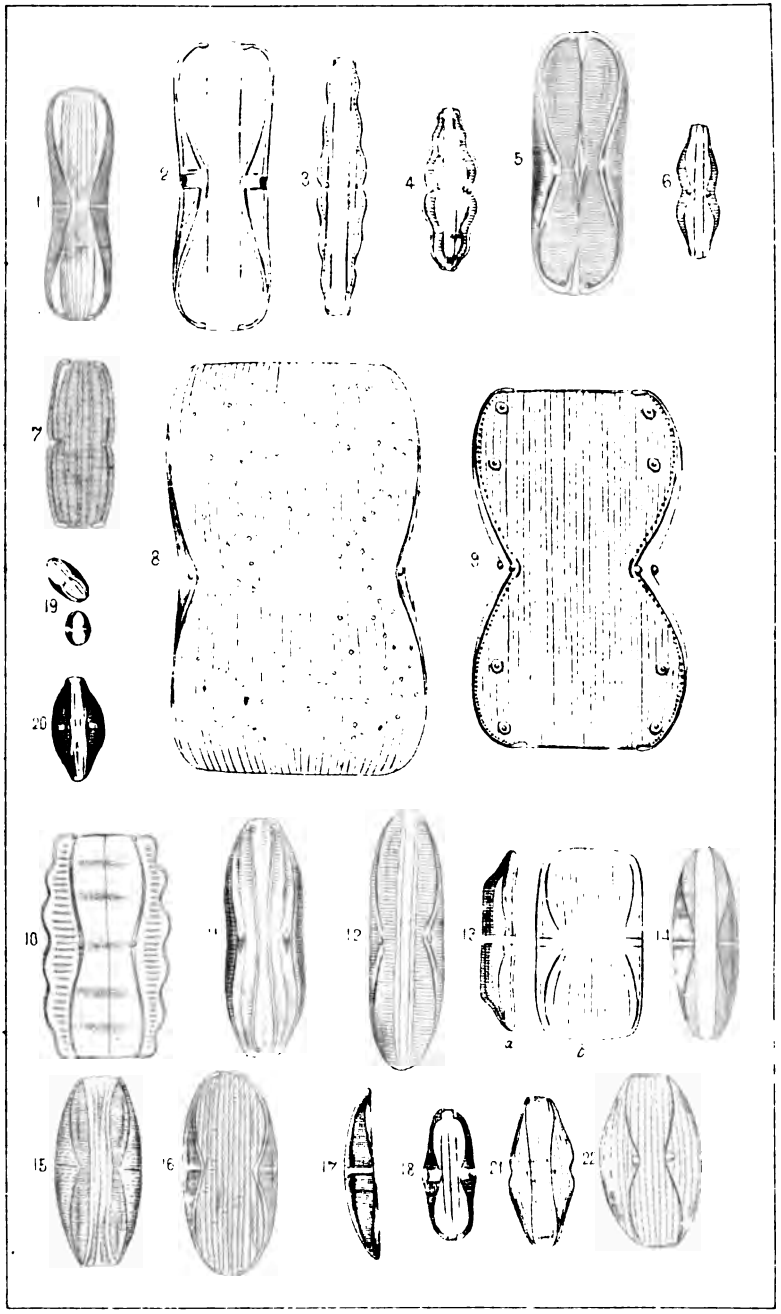
CUTANEOUS ABSORPTION OF POISONS.—In a recent note to the Paris Academy, M. Bernard describes a series of experiments for the purpose of testing the degree of cutaneous absorption which took place in a bath impregnated with the substances to be tested. Every precaution was taken to prevent the possibility of the substances entering the system of the patient by any avenue except the skin. He was then submitted for a short time to steam vapor charged with iodide of potassium, and two or three hours afterwards the urine gave unmistakable evidence that the iodide had been absorbed and was passing through the system.

In these experiments the medicinal agent reached the skin in hot aqueous vapor, and therefore acted more readily than an ordinary cold solution; but the fact of cutaneous absorption was very definitely illustrated. M. Bernard adds:

"M. Colin has described an experiment in which he allowed water charged with cyanide of potassium to fall for five hours on a horse's back. This caused the death of the animal; the sebaceous matter having been destroyed through percussion, and cutaneous absorption taking place."

COLORED SPECTACLES.—Dr. Stearns writes: "The photographer uses orange colored glass to exclude the actinic rays of light, and why some optician has not had the genius to see that orange is the proper color for spectacles, instead of green or blue, for persons with weak eyes, is beyond my comprehension. A room in the hospital with which I am connected is lighted through orange colored windows, and is used by patients who have certain diseases of the eyes requiring the exclusion of the actinic rays of light. It has been very satisfactory. Orange is also, I believe, the proper color for bottles containing chemicals affected by light."

AMPHORAE.



THE LENS;

A Quarterly Journal of Microscopy and the Allied Natural Sciences,

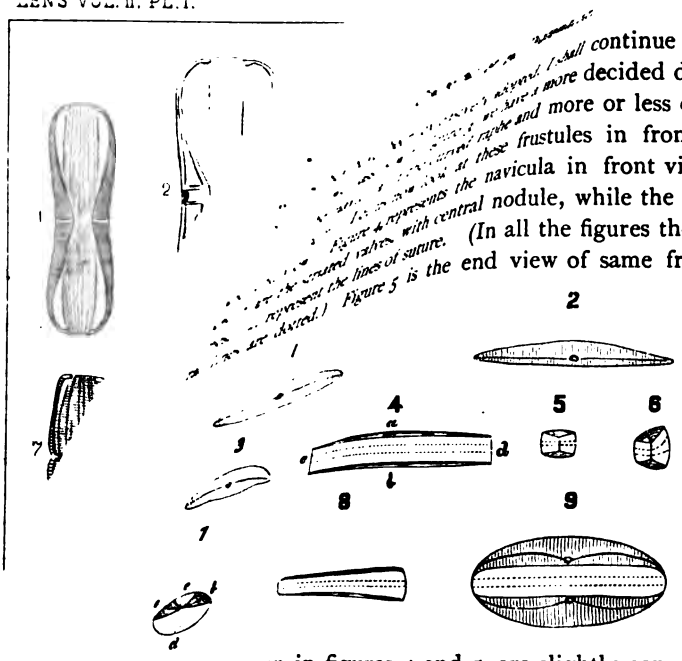
WITH THE

Transactions of the State Microscopical Society of Illinois.

VOL. II.—CHICAGO, APRIL, 1873.—No. 2.

CONSPECTUS OF THE DIATOMACEÆ.—ANALYSIS OF THE SPECIES OF THE GENUS AMPHORA.

A STRANGE misconception has existed as to the position of the genus Amphora; even so experienced an observer as Dr. Walker Arnott, while justly criticising the views of Gregory, Kützing, and W. Smith, himself falls into error; and he presents a very fanciful and false view of the structure of the frustules. Mr. Ralfs alone appears to have had a right conception, when he remarks that "Amphora contains several species of Agardh's genus Cymbella, and ought, in our opinion, to have retained that generic appellation." Although I have not united them with this genus, yet I have placed them in the family Cymbelleæ, and may hereafter consider them as a sub-genus, at least, of Cymbella. They are, in fact, exaggerated Cymbelleæ. Bearing in mind that all the diatomaceæ are built after the same type, or are siliceous boxes, as I have already indicated in the preface to the Synopsis, a reference to the following diagrams will make the structure of Amphora plain. If we commence with a typical navicula form, as in figure 1, presented in side view, we have the median line (*raphe*) dividing the valve symmetrically. Passing to figure 2, we have the typical Cymbella, the median line being nearer to one margin than the other, or dividing the valve unsymmetrically. The most convex margin is termed the *dorsum*, and the other the *venter*. Although these are objectionable terms,



continue to use more decided departmore or less curved frustules in front view navicula in front view; *a* nodule, while the dotted (In all the figures the sutuend view of same frustule.

While the valves, as seen in figures 4 and 5, are slightly convex, the *sutural zone*, or hyaline part which has upon it the sutural lines, is *of the same* width at the two ends *c* and *d*, figure 4, and again at *the middle* of the frustule, as seen in figure 5. Suppose now the *sutural zone* to become wider at one margin of the frustule, where it *passes* from figure 1 to figure 2, and widest at the middle of the dorsal surface, it would now appear as in figure 6, which is the end view of a Cymbella. We should still find the frustule, under action of gravity, lying upon one of its valves when allowed to fall freely, and so it would present itself generally in side view. Imagine now an excessive development of the sutural zone, as in figure 7 (which is an end view, as in figure 6), the frustule would no longer rest upon one of its valves, as in figure 6, but upon the expanded connecting zone between the two dorsal surfaces, and generally we would look down upon the frustule from *c* through to *d*, in which case both median lines *e f* would be in view, and if the median line incurved toward *c*, as it does in many species of the Amphoræ, we would now have the view presented in figure 9. Such is the simple structure of the frustules of this genus, which, through Cymbella, comes from Navicula.

I may here remark that in another group of diatomaceæ we have the same unequal development of the connecting zone, as in figure 8, front view, of a Gomphonema; here the greatest expansion is axial, or at one end of the median line, instead of equatorial, or at right angles to it, as in Cymbella and Amphora. This variation in the development of the connecting zone is not a distinctive feature, available for classification, for it would cause us to separate closely allied species, *e. g.* in Surirella, some of which are cuneate and some are not. The same remark applies to Diatoma.

The characters, then, which are available for classifying the Amphoræ, are mainly to be drawn from the aspect of the frustules in front view, as in figure 9; this, in fact, being the only position in which the frustule, if whole, will place itself when free to move. The following definitions will be found useful: The median line is either inflexed, as in all the figures in Plates I and II, or straight or slightly curved toward the outer margin, as in all the figures except 1–6 in Plate III. This gives us two great groups, and this feature is generally evident, even when but one valve is seen, in s. v. The space between the outer margin of the frustule and the raphe (or median line), is termed the outer portion of the valve, and in frustules with the incurved raphe, as in figure 9, is termed by Mr. Ralfs canoe-shaped; the inner margin of the valve is slightly curved or even straight, and generally (though not invariably) touches the nodule, the latter also generally touching (but not always) the connecting zone, upon which are the sutural lines, as shown by the dotted lines in figure 9. The outer portion of the valve is often striated or marked much more strongly (often only apparently) than the inner portion; the latter is sometimes obsolete, or, if present, very faintly striate or hyaline. The connecting zone between the nodules is frequently longitudinally striate, as is also the dorsal surface, or that portion which lies between the dorsal margins, and often these lines show through the valves. In describing the species, this feature of the markings of the frustules is used, but it must not be relied upon too strongly. In a few cases, the median line is so near the margin that the frustule is scarcely to be distinguished, except by the greater development in front view.

The student is advised to use the Synoptical Table rather than the Plates, in determining the position of any questionable specimen. Nothing can supply the place of concise descriptions.

GENUS I.

AMPHORA. *ἀμφὶ* and *φέρω*. E. 1840.

So called from resemblance to the ancient jar or urn, with two ears, or handles.

A. *Median line incurved* in f. v., Plate I, Plate II, and Plate III, figures 1–6.

- MARGINS constricted or undulate,
 - with stauros 1
 - without stauros,
 - hyaline 2
 - not hyaline,
 - with longitudinal lines ... 3
 - without longitudinal lines . 4
- MARGINS not constricted, or rarely, very slightly.
 - with stauros,
 - with rows of longitudinal lines,
 - margins straight 5
 - margins inflated 6
 - without longitudinal lines 7
 - without stauros,
 - with rows of longitudinal lines,
 - hyaline 8
 - not hyaline,
 - margins straight, 9
 - margins inflated, 10
 - without longitudinal lines,
 - hyaline 11
 - not hyaline,
 - margins straight or gibbous, 12
 - margins inflated, 13

B. *Median line not incurved* in f. v., Plate III, figures 6–37.

- MARGINS constricted,
 - with stauros 1
 - without stauros 2
- MARGINS not constricted, or rarely, very slightly.
 - with stauros,
 - median line curved 3
 - median line straight 4
 - without stauros,
 - rostrate,
 - with longitudinal lines on valves,
 - median line curved, 5
 - median line straight, 6
 - without longitudinal lines on valves,
 - median line curved, 7
 - median line straight, 8
 - not rostrate,
 - median line curved 9
 - median line straight 10

LIST OF ABBREVIATIONS.

A. N. H.—Annals and Magazine of Natural History, London.
A. N. S. P.—Academy Natural Sciences, Philadelphia.
B. C.—Bailey. Smithsonian Contributions, 1850.
B. M. O.—Bailey. Smithsonian Contributions, 1854.
B. J. N. H.—Loring W. Bailey, Boston Journal of Nat. History, Vol VII, No. iii. Two plates.
B. Ch.—Note sur quelques Diatomees Marines par M. Alphonse De Brébisson, Cherbourg.
C. S. & N. D.—Svenska och Norsk a Diatomacéer Af P. T. Cleve, öfversight af K. Vet. Akad Förhandl. 1868, T. IV.
C. S. D.—Diatomacéer från Spetsbergen Af P. T. Cleve, öfversight af K. Vet. Akad. Förhandl. 1867, T. XXIII.
E. A.—Ehrenberg. Verbreitung und Einfluss des Mikroskopischen Lebens in Süd und Nord-Amerika, 1843.
E. M.—Mikrogeologie von Christian Gottfried Ehrenberg, 1854.
E. R. B. A.—Ehrenberg. Reports, Berlin Academy.
F. E. A.—Flora Europaea Algarum. Dr. L. Rabenhorst, 1864.
G. D. C.—On new forms of Marine Diatomaceæ found in the Firth of Clyde and in Loch Fine. Wm. Gregory, M. D.; F. R. S. E.
G. S. P.—New Genera and Species of Diatoms from the South Pacific. Parts I, II and III, by R. K. Greville, L. L. D.; F. R. S. E., &c. Ed. New Phil. Journal, Vol. XVIII.
G. D. H.—Diatomeen auf Sargassum von Honduras, gesammelt von Lindig, untersucht von A. Grunow.
GRUN.—Ueber einige Neue und Ungenügend bekannte Arten und Gattungen von Diatomaceen, von A. Grunow, 1836.
K. B.—Die Kieselschaligen Bacillarien von Dr. Fr. Traug. Kützing, 1844.
K. S. A.—Species Algarum *Auctore*, Fr. Traug. Kützing, 1849.
M. J.—Quarterly Journal, Microscopical Society, London.
N. D.—Reise seiner Majestät Fregatte Novara. Botanischer Theil, Algen von A. Grunow, 1868.
R. S. D.—Die Süsswasser-Diatomaceen, Dr. L. Rabenhorst, 1853.
S. B. D.—A Synopsis of the British Diatomaceæ, by the Rev. Wm. Smith, *F. L. S.* Vol. I, 1853. Vol. II, 1856.
Sch.—Preussiche Diatomeen. 1862, 1865, 1867. J. Schumann.
T. M.—Transactions Microscopical Society, London.

SECTION A.

I.

1. *A. lævis.* Greg. G. D. C. Pl. IV, f. 74.

Rectangular, slightly incurved at the middle, ends rounded or nearly square. Marine. Length .0017″ to .0032″, breadth .0007″ to .0012″; striæ transverse, 60 in .001″. (I. f. 1.)

2. *A. ocellata.* Donk. M. J., Vol. I, N. S, Pl. 1, f. 11, b.

Rectangular, ends rounded, middle slightly incurved. Stauros a hyaline band, causing a dark spot apparently on each margin, at its end. Marine. Length .0028″, breadth about .001″. (I. f. 2.)

II.

3. *A. flexuosa.* Grev. G. S. P. II, Pl. IV, f. 4.

Frustules with six gentle undulations, and marginal row of minute punctæ. Nodules at the angle of the constriction. Marine. Length .0034″. (I. f. 3.)

4. *A. undata.* n. s.

Doubly lyrate, sharply and somewhat angularly constricted at the middle. Nodule distinct, valves with several longitudinal lines inflexed like the margins of the frustule, and convergent at the ends; inner margins of valves slightly curved; connecting zone with longitudinal lines. Marine. Length .0017″, breadth .00075″, transversely striate; striæ fine, about 55 in .001″, dry frustule, straw color. (I. f. 21.)

This pretty form associated with *A. aponina, Amphiprora pulchra, Bacillaria paradoxa* and *Nitzschia Sigmoidea* was found in brackish ponds near New Haven, Conn. The longitudinal lines are often much more strongly shown than in our figure.

5. *A. obtusa.* Greg. T. M. v. Pl. 1, f. 34.

Frustules broad, ends rounded, length .0037″ to .004″, constriction slight, striæ fine, 70 in .001″. (I. f. 5.)

III.

6. *A. Milesiana.* G. D. C. Pl. v. f. 83.

Nearly rectangular, with constriction at middle, between which and the ends, the margin is a little convex, several longitudinal bars between the lateral segments, nodules marginal. Marine. Length .0023″, breadth .001″, striæ conspicuous, 28 in .001″. (I. f. 7.)

7. *A. Magnifica.* Grev. G. S. P. II, Pl. IV. f. 1.

Frustules large, rectangular, slightly constricted, outer portion narrow, only visible at the middle, nodule minute; dorsum with numerous longitudinal lines, about 10 in .001″, and brilliant scattered punctæ. Marine. Length .004″ to .0055″. (I. f. 8.)

8. *A. complanata.* Grun. G. D. H. No figure.

Rectangular, ends slightly rounded, frustules with numerous longitudinal lines, and small lanceolate points; slightly constricted, resembling Greville's *A. magnifica*, but smaller, and without the remarkable punctæ on the longitudinal lines. Marine. Striæ very fine. Grunow gives no figure.

9. *A. pulchra.* Greville. G. S. P, II. Pl. IV. f. V.

Large, panduriform, outer portion narrow, dorsum with numerous longitudinal lines, and four brilliant sub-marginal punctæ on each side. Marine. Length .004″ to .005″. (I. f. 9.)

IV.

10. *A. binodis.* Greg. G. D. C. Pl. IV, f. 67.

Frustules deeply constricted. Marine. Length .00175″ to .002,″ striæ obscure, about 30 in .001″. (I. f. 6.)

11. *A. sarniensis.* Grev. T. M. Vol. II, N. S. (X) Pl. IX, f. 12.

Frustules sharply constricted, lobes with a double undulation, ends produced, truncate. Marine. Length .0017″ to .0022″, striæ 30 in .001″. (I. f. 4.)

12. *A. undulata.* Grev. G. S. P. II. Pl. IV, f. 3.

Frustules rectangular, with four sub-equal inflations. Marine. Length .003″, striæ coarse, 14 in .001″. (I. f. 10.)

13. *A. proboscidea.* Greg. G. D. C., Pl. VI, f. 98, 98 *b.*

Frustules nearly rectangular, narrower at the ends, which are truncate and slightly produced, constriction of margin slight; inner margin of valves bent outwards at the ends. Marine or brackish. Length .003″ to .005″, breadth .001″ to .0015″; outer portion coarsely striate, 20 in .001″, inner portion hyaline. (I. f. 11.)

This diatom appears to be *A. affinis* of W. S. as figured S. B. D. Pl. II, f. 27, and which is not *A. affinis* of K.

14. *A. oblonga.* Greg. G. D. C. Pl. v, f. 78. 78 *b.*

Linear elliptic, ends obtusely acuminate, constriction of margin very slight, central nodules conspicuous. Marine. Length .0034″ to .004″, breadth .001″ to .0014″. (I. f. 12.)

V.

15. *A. kamorthensis.* Grun. N. D. T. I, A. f. 12, *a b c.*

Frustules sub-rectangular or slightly constricted in the middle, ends rounded, valves with a longitudinal furrow parallel with the dorsal margin, ends produced in s. v. connecting membrane with longitudinal lines. Marine. Striæ punctate, subradiate, 35 in .001, smaller or obsolete in the space between the median line and the longitudinal sulcus. (I. f. 13.)

Formerly described by Grunow as *A. Grevilliana,* from which, however, it is quite distinct.

16. *A. vitrea.* Cleve. C. S. & N. D. T. IV, f. 5, 6.

Rectangular, with rounded corners; connecting zone with evident longitudinal lines, ventral margin of valves concave, dorsal margin convex in s. v. Marine. Length .004″, striæ about 22 in .001″. (II. f. 1.)

VI.

17. *A. litoralis.* Donk. T. M. VI, Pl. III, f. 15. 15 *b.*

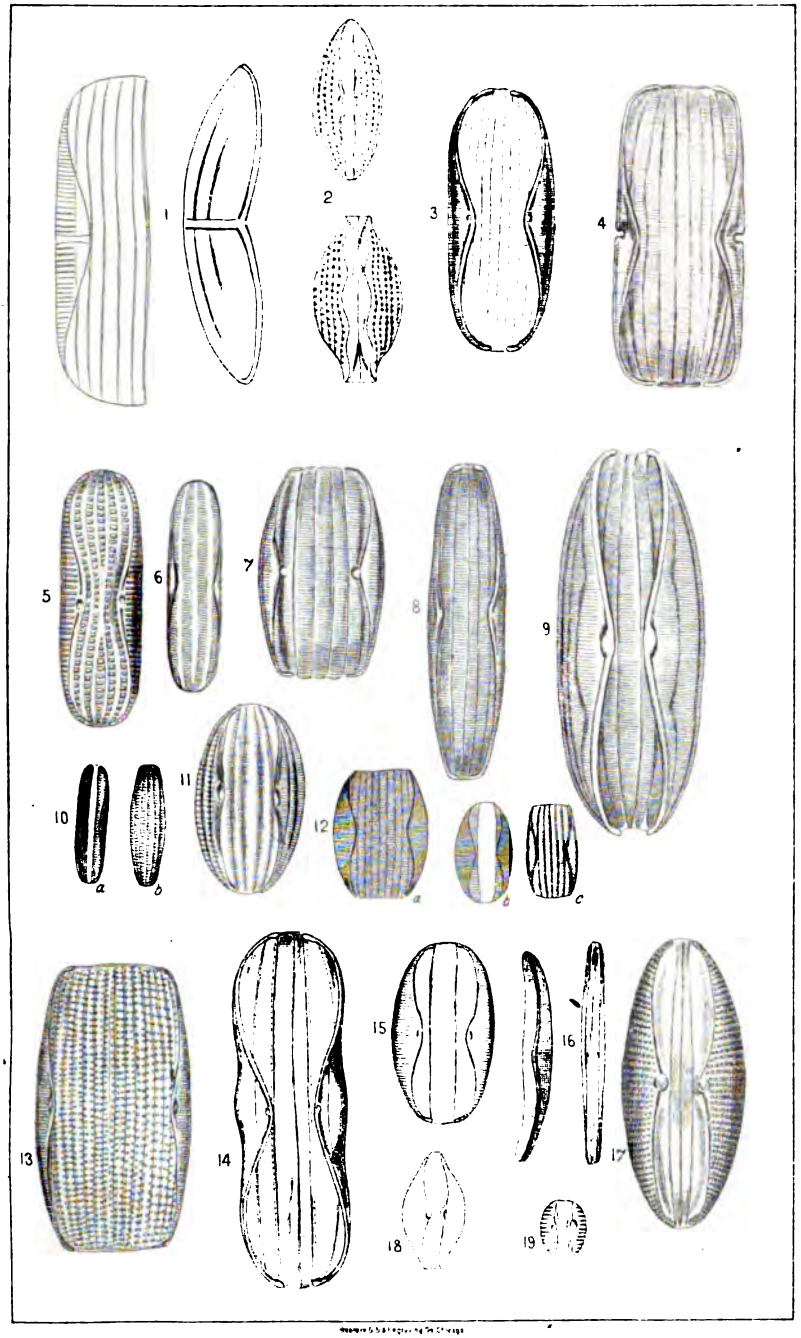
Oval, with truncate ends; connecting zone marked with several longitudinal lines of linear, transversely set punctae. Marine. Length .002″ to. 003″, breadth .0008″ to .0012″; striæ distinct, moniliform, finer on the inner compartment. (I. f. 15.)

18. *A. ostrearia.* Breb. in K. S. A., p. 94.

Frustules elliptic-oblong, with regularly rounded ends, connecting zone with numerous fine longitudinal lines. Marine. Length .0013″ to .003″, finely striate, 80 in .001″. (I. f. 16.)

A. quadrata, Breb., differs from *A. ostrearia* only in its straight margins, as stated K. S. A. p. 95. Rabenhorst unites them, *Flora Europaea Algarum,* Sectio I, p. 88. *A. membranacea* of W. S. also belongs here, the figure by Roper, M. J. VI, Pl. 3, f. 8, *a, b,* is very good. Grunow states that it is very variable in striation, he having found specimens with 40 to 60 in .001″. *A. elegans* of Gregory T. M. v. Pl. 1, f. 30 (I. f. 17) appears to be same as *A. ostrearia* and

AMPHORAE



A. membranacea, but since the striæ, as he states, are more readily seen, and as he makes no mention of the very distinct longitudinal lines, I have not united them.

VII.

19. *A. elegans.* Greg. T. M. v. Pl. I. f. 30.

Frustules elongate, with somewhat truncate extremities, stauros more distinct than in *A. ostrearia*, and striation, though fine, more readily seen, connecting membrane very transparent, length .001″ to .0025″. (I. f. 17.)

20. *A. decussata.* Grun. G. D. H. No figure.

Valves semi-lunate, ventral surface straight, or sub-concave, and slightly biundulate; dorsum convex; a longitudinal line near ventral margin of the valve; narrower and inner portion of valves transversely striate, outer portion obliquely, decussating with distant, interrupted, somewhat obsolete and variously curved lines; under careful examination the oblique lines are perceived to arise from elongated connected cellules; dorsal margin strongly punctate. Marine. Length .0027″ to .0065″, breadth .0007″ to .0012″, striæ 40 to 48 in .001″.

Grunow gives no figure; he says, "this diatom under name of *A. Stauroneiformis* was first observed by Dr. Lorenz; a name not suitable as it belongs to a whole group;" he further remarks that he has never seen the whole frustule, but has no doubt that in front view is very similar to *A. ostrearia* which indeed, is sometimes represented with diagonal markings.

21. *A. lævissima.* Greg. G. D. C., Pl. IV, f. 72.

Elliptic, rather narrow, and very hyaline. Curve of median line gentle, striæ exceedingly fine and seen with difficulty. Length .0025″ to .003″. (I. f. 14.)

22. *A. delphina.* L. W. B.; B. J. N. H., VII, Pl. I, f. 1.

Elliptic, oblong, ends broad, slightly rounded, center gibbous, nodules large, extending in a bar across the valve, terminal nodules distinct, finely striate. Marine. (I. f. 18.)

A. minutissima, W. S.; S. B. D., pl. II, f. 30. (I. f. 19.)

A. Libyca. Ehr. E. M. *passim.* (I. f. 20.)

A. gigas. Ehr. E. M., Pl. VI, 2 f. 13.

The three last named species are varieties of *A. ovalis.* As for *A. minutissima*, I have never seen it with finer striæ than about

40 in .001″, instead of 64 in .001″, as stated by W. Smith; it is quite common, parasitic as well as free, associated with others, ranging from these minutest to the normal size. Ehrenberg's figures of *A. Libyca* are about as unlike as anything purporting to be a representation of the same thing, could be. Of *A. gigas*, only a fragment is represented, no way differing from *A. ovalis*. I place these three names here on account of the pseudo-stauros. Indeed, with the smaller forms always, and sometimes with the larger, the central nodule, under such focal adjustment as exhibits best the outlines of the frustule, will expand into an apparent stauros. All three are fresh water forms.

VIII.

23. *A. lineolata.* Ehr., Raben. R. S. D., T. IX, f. 9, 10.

Inflated, ends truncate, nodules distinct, outer portions with strong, and inner with fine, longitudinal lines; fresh water; length .002″ to .004″. (I. f. 22.)

Ehrenberg, Kützing, and Donkin, have given this name to three different species of Amphora, and Ehrenberg himself, has again given it to forms which are true *A. ovalis;* e. g. in the Lüneburg deposit; and again in E. A. he has figured an entirely distinct marine form under same name. Kützing's form is probably a variety of *A. coffeæformis*, and Donkin's name was changed by Rabenhorst to *A. Donkinii.* According to Grunow, *A. lineolata* of E., has 70 striæ in .001″. He remarks that "it is found in salt and brackish water, varying considerably in size and coarseness of striation, as do most species of Amphora. The largest and strongest marked forms have 36 to 40 striæ in .001″, and answer to Gregory's representation of *A. sulcata* Bréb."

24. *A. hyalina.* K. B. Pl. 30, f. 18. W. S., S. B. D., Pl. II, f. 28.

Hyaline, elliptic lanceolate, with acute or truncate apices, and a few delicate, longitudinal lines; central nodule often very small, median line feebly bow-shaped, valves without color when dry, and often imperfectly siliceous. Marine. Finely striate, about 60 in .001″. (II. f. 2.)

A. hemispherica. Grun. G. D. H. Grunow described this as a new species having fine transverse striae, 55 to 60 in .001″, with distant, interrupted, obscure longitudinal striae; subsequently he became convinced that it was *A. hyalina*; see *A. crystallina.*

25. *A. plicata.* Greg. T. M., v. Pl. 1, f. 31.

Rectangular, broad, corners rounded, median line deeply curved, outer portion of valves faintly marked with transverse striæ difficult to be seen; median space marked by strong longitudinal, slightly curved lines, which appear as folds. (II. f. 3.)

IX.

26. *A. excisa.* Greg. G. D. C., Pl. v. f. 86.

Rectangular, median line near the outer margin except at the middle, where it bends inward to a nodule; on the outer margin is another, larger and more conspicuous, causing it to appear deeply notched at this point. Marine. Length .0028″ to .004″, breadth .0015″. Connecting zone with a number of convergent longitudinal bars; valves hyaline, finely striate, about 52 in .001″. (II. f. 4.)

27. *A. biseriata.* Greg. T. M., v., Pl. 1, f. 32.

Rectangular, corners rounded, margins somewhat incurved, median line not conspicuous, projecting very little from the margin in the middle point; connecting zone with longitudinal bands of short transverse striæ. Marine. Length .003″ to .0045″, striae about 18 in .001″. (II. f. 6.)

I have doubts whether this is an Amphora; it has much the appearance of Lewis' *N. Powelii* in f. v.; the blank spaces, however, are much more strongly marked.

28. *A. pusilla.* Greg. G. D. C., Pl. 6, f. 95.

Small, linear, with rounded ends, nodule and median line near the margin; lateral segments narrow. Between the lateral segments are several narrow bars separated by fine sharp lines, and marked by sub-distant granules, or short striæ. Marine. Length .0014″ to .0021″, breadth .0004″ to .0006″, striæ conspicuous, 24 in .001. (II. f. 10.)

29. *A. crassa.* Greg. G. D. C.=*A. sulcata.* Breb. See below.

X.

29. *A. sulcata.* Bréb. B. Ch., f. 8.

Oblong, or elliptic-oblong, or sometimes slightly incurved, connecting zone with longitudinal lines of transverse striæ. Marine. Length .0041″, breadth .002, valves striate. I give Brébisson's figure. (II. f. 11.)

Grunow remarks, G. D. H., p. 14, that "the largest and strongest marked forms of *A. lineolata*, *E.*, answer to Gregory's representation of *A. sulcata*, which latter appears to be a more strongly marked species, judging from Brébisson's only figure, and is perhaps *A. crassa*, Gregory." Mr. Roper appears to have first noticed, in England, the form to which Prof. Gregory afterwards gave the name "*crassa*," and he figures it in M. J. VI, Pl. III, f. 7, as *A. sulcata* for he was unwilling to add, upon doubtful grounds, another to the long list of native species. The following is a description of this diatom:

A. crassa. Greg. G. D. C., Pl. VI, f. 94.

Rectangular, broad, ends rounded, dorsal margin sometimes slightly incurved, five to eight longitudinal convergent bars between the lateral segments, marked like the valves with sub-distant, entire striæ. Marine. Length .0025″ to .004″, breadth .0008″ to .0013″, striæ coarse, about 12 to .001″. (II. f. 5.)

Certainly Gregory's figure of *A. crassa* and Brébisson's of *A. sulcata* are not remarkably alike, but as there is little doubt that Grunow is right in placing them together, we may judge somewhat of the value of these pictorial repesentations of, too often, the fancy of the observer. Gregory's figures, however, made by Greville are nearer a true representation, but this diatom is represented, as it appears to me, too coarsely. Mr. Roper's figure, *l. c.*, is better. I have this species in a gathering from Cherbourg, and have little hesitation in uniting them as Grunow suggests. I cannot, however, include the much finer marked form, *A. plicata*, which he also thinks belongs to *A. sulcata.* Brébisson's remark that his diatom differs from *A. costata*, W. S. by the latter having produced apices, would indicate that *A. sulcata* was coarsely striated. I would, however, include

A. truncata. Greg. G. D. C., Pl. V, f. 77.

Frustules barrel-shaped, with truncate ends, median line bending gently inwards to a small nodule; space between the valves broad, with longitudinal lines, or bands, of short striæ; valves transversely striate, more conspicuous at the margin. Marine. Length .0028″.

30. *A. elongata.* Greg. G. D. C., Pl. V, f. 84.

Elliptic, lanceolate, long, narrow, with truncate extremities; lateral segments very narrow, median line near the outer margin,

connecting zone with several convergent longitudinal bars. Marine. Length .0044″, breadth .0011″, striæ conspicuous, transverse, 26 in .001. (II. f. 8.)

31. *A. quadrata.* Greg. G. D. C., Pl. v, f. 85.

Nearly rectangular, sides slightly convex, ends truncate; several broad longitudinal bars which are striated on the connecting zone, between the nodules. Marine. Length about .0007″, breadth .0018″, valves transversely striate; striæ 34 in .001″. (II. f. 7.)

A. Gregorii. Ralfs. The name *quadrata* having already been given to an Amphora by Brébisson, Mr. Ralfs altered the name of this species to *A. Gregorii*, but as already noticed under *A. ostrearia*, Brébisson's form is only a variety of that species, and so Prof. Gregory's name can be retained. I am not certain but that it is one the varieties of the next group.

32. *A. Grevilliana.* Greg. G. D. C., Pl. v, f. 89.

Frustules broad, barrel-shaped, dorsum with a longitudinal series of somewhat convergent bars, composed of striæ. Marine. Length about .005″, breadth about .002″. (II. f. 9.)

A. fasciata. Greg. G. D. C., Pl. v, f. 90.

A. complexa. Greg. G. D. C., Pl. v, f. 91.

Mr. Ralfs has already, very properly, united these with *A. Grevilliana.*

33. *A. Arcus.* Greg. G. D. C., Pl. v. f. 88.

Frustules barrel-shaped, ends truncate. Marine. Length .0035″ to .0045″, breadth about .002″. Valves coarsely moniliform striate. Striæ 16 to 18 in .001″. (II, f. 13.)

34. *A. obtecta.* J. W. B., L. W. B.; B. J. N. H., VII, Pl. II, A.

Frustules barrel-shaped, with straight truncated ends, nodules wanting or obscure, whole frustule covered with close transverse striæ, which in f. v. intersect fine longitudinal lines or folds in the connecting membrane, giving the frustule the appearance of being woven all over; marinè. (II, f. 12, *a b c.*)

Prof. L. W. Bailey states in a letter to me, that the figure, though a fac-simile of the drawing left by his father, who found this diatom in soundings off the coast of Brazil, is not satisfactory. Probably it is only a variety of *A. sulcata.*

XI.

35. *A. Semen.* Ehr. E. M., Pl. 38, 17, f. 10.

Inflated, with broad, shortly produced truncate ends, without striæ, (?). (II. f. 18.)

This is another of Ehrenberg's unsatisfactory species. The associated diatoms are fresh water forms, and the figure is very imperfect.

36. *A. pellucida.* Greg. G. D. C., Pl. IV, f. 73, 73, *b.*

Broad oval, delicate and transparent, nodules distinct, median line strongly inflexed, ventral margin excessively hyaline, outer portion of valves striate. Marine. Length .002″ to .003″, breadth .0012″ to .0018″, striæ shallow, 30 in .001. (II. f. 15.)

Rabenhorst considers this as a variety of *A. ovalis*, for which it might pass were it not for the marine habitat, and singular delicacy of the striæ; Ehrenberg's figure of *A. lineolata* from the Lüneburg deposit, and which is *A. ovalis*, is exceedingly like this.

A. incurva. Greg. M. J., III, Pl. IV, f. 6.

A. Erebi. Ehr. E. M., 35 A. 23, f. 3.

Single valves of these two species are figured; they appear to belong here. The description of *A. Erebi* is "lateral view arcuate, with obtuse apices, concave venter, and about 30 striæ in .001″." E. R. B. A. 1853, p. 526. The valve as figured, has a strongly arcuate raphe, exactly like the valves of *A. pellucida.*

37. *A. arenaria.* Donk. M. J., VI, Pl. III, f. 16.

Frustules hyaline, rectangular, colorless, extremities slightly rounded, sides somewhat uneven, slightly bulged out at the middle, and at the extremities. Dorsal surface faintly marked with six to eight longitudinal lines, the outer converging at the extremities; exceedingly transparent, and marked with transverse, very delicate moniliform striæ. Marine. Length .004″ to .006″, breadth about .0016″. (II. f. 14.)

38. *A. inflexa.* H. L. S. *Amphipleura inflexa.* Bréb. K. S. A.

Linear, valves arcuate, with rounded extremities; median lines distinct, nodules very minute, whole frustule hyaline. In f. v. median lines inflexed and touching the sutural lines; marine. (II. f. 16.)

There has been always some doubt of the propriety of placing this diatom among the *Amphipleuræ.* I received specimens from De Brébisson just before his death marked "not an *Amphipleura*; the

proper genus is *Toxonidia.*" Doubtless the inflexed median line induced him to place it in this genus. Grunow thinks it should be the type of a new genus.

XII.

39. *A. naviculacea.* Donk. M. J., Vol. I, N. S. Pl. I, f. 12.

Rectangular, extremities slightly rounded,, striæ on dorsal or outer half of the valve continuous, and nearly parallel; on the inner or ventral half coarser, and absent opposite the central nodule; strongly divergent on either side of it, and convergent near the terminal nodules; space between sutural lines blank. Marine. Length about .0032″, breadth .0011″. (III. f. 4.)

Donkin's figure in M. J. is not numbered on the plate.

40. *A. Donkinii.* Rab. F. E. A.=*A. lineolata.* Donk. M. J. I. N. S. Pl. I, f. 13.

Nearly rectangular, slightly convex laterally, hoop with several longitudinal plicæ, median line gently incurved. Marine. Length about .003″, breadth .0012″, finely striated transversely, striæ delicate. (III. f. 5.)

The name *lineolata* having already been appropriated by E. for another species of Amphora, Rabenhorst changed the name of this species to *Donkinii.* The figure is not numbered on the plate in M. J.

XIII.

41. *A. Proteus.* Greg. G. D. C., Pl. V, 81, 81*b*, 81*c*, 81*d*, 81*e*.

Somewhat variable in outline, lanceolate, elliptical, barrel-shaped, or truncate. Inner margin of raphe or median line, marked with longitudinal moniliform lines or bars. Marine. Length .0015″ to .006″, breadth .0013″ to .0024″, outer compartments distinctly striate, about 22 in .001″. (III. f. 1.)

The name indicates the variable appearance both as to size and outline of this form, which, were it not for the marine habitat, might pass for *A. ovalis.* It is exceedingly abundant on the Atlantic coast, and a variety from Florida, found also in New Jersey, shows a strong line limiting the striæ between the median line and the margin.

42. *A. spectabilis.* Greg. G. D. C., Pl. v, f. 80, 80*b*? 80*c*?

Nearly rectangular, broad, with rounded ends, and occasionally sub-elliptical; aspect of the whole form soft and indistinct, so that in general only the marginal ends of the striæ can be easily seen; with high powers and careful adjustment of focus, the dorsal surface is found to be marked with longitudinal bars of fine striæ, 50 in .001″. Marine. Length .003″ to .0047″, breadth from .002″ to .0025″, outer portion of valves striate, 14 to 16 in .001″. (III. f. 3.)

A. dubia. Greg. G. D. C., Pl. v, f. 76.

This appears to be a frustule in act of self-division, belonging to one of the preceeding species, perhaps *A. spectabilis*, the striation is 24 in .001″. From the occurrence of longitudinal bars, composed of fine striæ, and which in the case of *A. spectabilis*, are not visible unless under high power, and careful focussing, Dr. Gregory concludes that this species belongs to his group of complex Amphoræ! These bands of longitudinal striæ are not uncommon on the connecting zone of a great many diatoms, when usually it has been considered structureless, e. g. in the large *Amphiprora pulchra*, they are well shown. I need hardly remark that it is no evidence of a complex structure. Dr. Gregory has drawn largely on fancy.

43. *A. robusta.* Greg. G. D. C., 79, 79*b*.

Broad oval, with sub-truncate extremities, frustules thick, marked with strong striæ; in the outer compartments transverse, in the inner somewhat radiate. Marine. Length .003″ to .0048″, breadth .0018″ to .0024″, striæ sub-distant, moniliform, about 16 in .001″. (III. f. 2.)

44. *A. angusta.* Greg. G. D. C., Pl. IV, f. 66.

Small, rectangular, linear, elliptical, narrow, ends truncate, valves transversely striate both sides the median line. Marine. Length .0015″, breadth .0004″, striæ fine, 44 in .001″. (III. f. 6.)

45. *A. ovalis*, K. K. B., Pl. v, f. 25. S. B. D., Pl. II, f. 26.

Turgid oval, with broadly rounded or truncate ends, striæ monilform, distant, 24 in .001″. Fresh or brackish. (II. f. 17.)

A. elliptica. Rab. Elliptic, gradually attenuated towards the ends.

A. gigas. Ehr. E. M. T. 4, 2, f. 13.

A. globulosa. Sch. Preuss. Diat. T. 1, f. 25. (II. f. 19.)

A. minutissima. W. S.; S. B. D., Pl. II, f. 30. (I. f. 19.)

AMPHORAE.

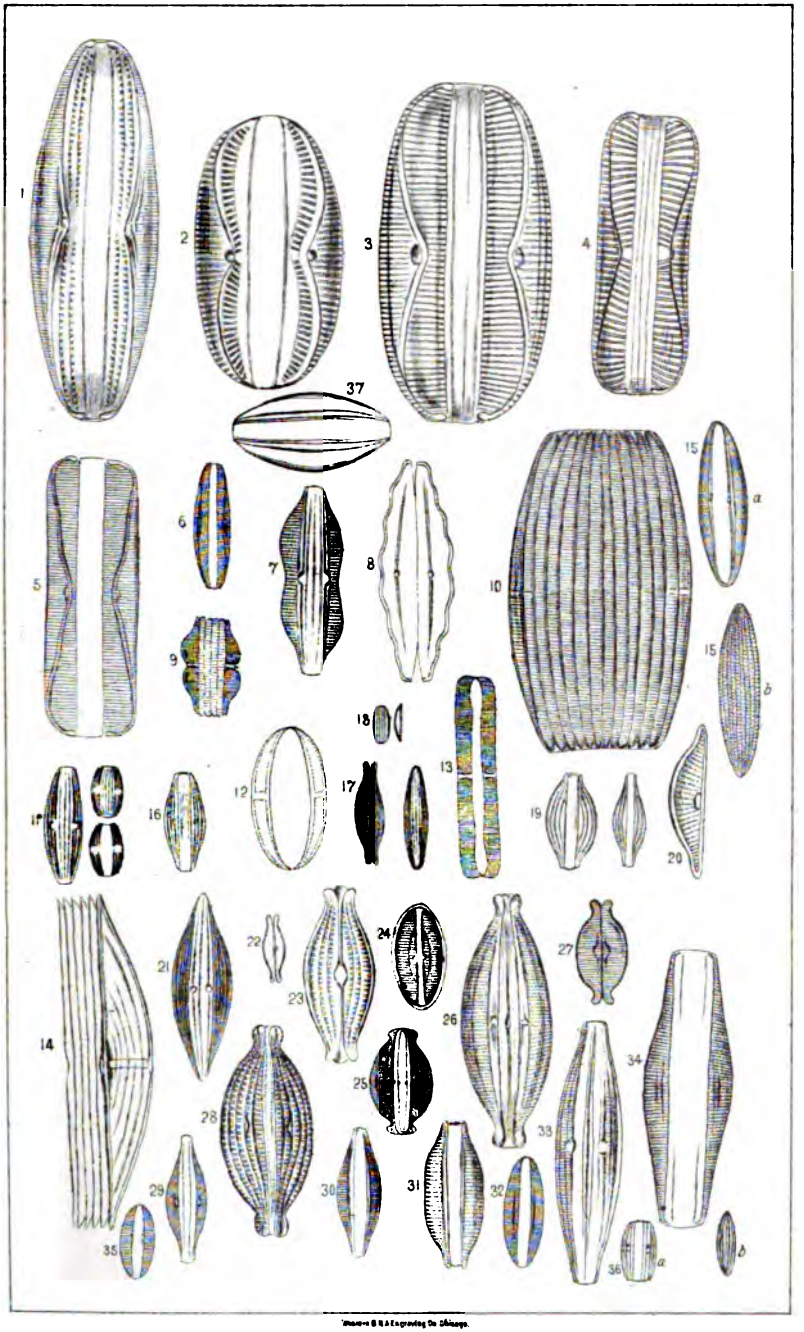


Photo-Engraving Co. Chicago.

A. Normani. Rab. F. E. A., p. 88=*A. minutissima*, from warm baths near Hull.

A. nana. Rab. Alg. *Sub. No.* 765. Long, ovate, with round apices.

A. abbreviata. Bleisch in Raben. Alg. *Sub. No.* 1489. Small, smooth, constricted at ends.

A. Libyca. Ehr. E. M. *passim.* E. A. Pl. III, I, f. 42, and VII, f. 17.

SECTION B.

I.

46. *A. lyrata.* Greg. G. D. C., v. f. 82.

Doubly lyrate, ends truncate, whole form transversely striate, four or five longitudinal bars between the lateral segments. Marine. Length .0011″, breadth .00075″, striæ fine, 36 in .001″. (III. f. 9.)

II.

47. *A. angularis.* Greg. M. J., III, Pl. IV, f. 6.

Sinuato-constricted at the middle, with short, broadly linear, truncate produced ends; striæ distinct. (III. f. 7.)

48. *A. sinuata.* Grev. G. S. P., II, Pl. IV, f. 5.

Narrow, oblong elliptical, with truncate, shortly produced ends; undulations six, the two middle ones largest; nodules considerably within margin; striæ obscure. Marine. Length .0028″. (III. f. 8.)

III.

49. *A. rimosa.* Ehr. E. M., Pl. 13, 2, f. 17.

Elliptic-oblong, ends rounded, hyaline, (?) with a strongly developed stauros. (III. f. 12.)

Like many of Ehrenberg's figures, this is exceedingly unsatisfactory; probably it will prove a variety of some other species.

50. *A. nobilis.* Greg. G. D. C., Pl. v, f. 87.

Broad, ends truncate, somewhat hyaline, with numerous converging longitudinal bars in the middle. Marine. Length .0013″ to .0028″, striæ fine, about 40 in .001″. (III. f. 10.)

IV.

51. *A. acuta.* Greg. G. D. C., Pl. v. f. 93. Pl. VI, f. 93*b*.

Elliptical, with extremities slightly produced, connecting zone with strong longitudinal lines, outer portion of valves moniliform

striate. Marine. Length .0035″ to .0055″, breadth .00075″, striæ about 36 in .001″. (III, f. 14.)

Gregory gives no front view of this species, but remarks that probably it is like *A. nobilis*, from which it differs by the inner margin being straight, and valves stronger moniliform striate; it is quite probable they may be varieties of same species.

52. *A. rectangularis.* Greg. T. M., v, Pl. 1, f. 29.

Nearly rectangular, slightly constricted, ends rounded, their margins slightly undulate, hyaline part of connecting zone widest at the ends, length .0025″ to .0045″, valves transversely striate, 40 in .001″. (III. f. 13.)

This is doubtfully an Amphora; Prof. Gregory placed it here on account of the stauros; probably it is a (*Navicula*) *Stauroneis.* I have found it associated with *A. plicata* in gatherings from Atlantic City.

V.

53. *A. coffeæformis.* Kütz. K. B., Pl. v, f. 36.

Elliptic oblong or lanceolate, turgid at the middle, apices somewhat elongated, truncate; longitudinal lines on the marginal, and very fine ones on the central, portion; length .0012″ to .0021″. (III. f. 17.)

A. lineolata. Kütz. K. B., Pl. 5, f. 36. (III. f. 16.)

A. Fischerii. Kütz. K. B., Pl. 5, f. 38. (III. f. 19.)

It is already conceded by Ralfs, Rabenhorst, and others, that *A. lineolata* of Kützing is not *A. lineolata* of E., and that *A. Fischerii* should be united with *A. coffeæformis.* *A. Hohenackeri*, and *A. quadricostata* differ in having a straight median line, and apices not produced, yet little reliance can be placed on the figures, and they may prove to belong here. Rabenhorst gives also *A. acutiuscula* as a synonym; it appears, however, to be a Navicula. He also quotes as synonym *A. exigua*, Greg., which is much more coarsely striate, 28 in .001″, and also *A. lineata*, Greg., which I have considered an entirely distinct form.

54. *A. lineata.* Greg. G. D. C., Pl. IV, f. 70.

Elliptic or elliptic-lanceolate, with short produced apices, which are truncate; outer portion of valves marked with strong longitudinal lines. The whole frustule has a characteristic linear aspect.

Marine. Length .0022″ to .003″, breadth .0007″ to .008″, finely striate, 42 in .001. (III. f. 21.)

I am by no means sure that Rabenhorst is not right in uniting this with *A. coffeæformis*; it is closely allied to the next species, and with it is not uncommon in gatherings from the Atlantic coast.

55. *A. costata.* W. S. S. B. D., Pl. xxx, f. 253.

Frustules ventricose, with short, broad, truncate beaks, longitudinally costate; transversely striate, 16 in .001″, according to Prof. Gregory; my own measures give 24 in .001″, and Smith's figure appears altogether too coarse. I found it in brackish ponds, N. Haven. Marine, or brackish. Length .002″ to .0033″, breadth .0012″ to .0016″. W. Smith's figure. (III. f. 28.)

56. *A. Terroris.* E. E.R.B.A., 1853, p. 156. E. M., 35 A. 23, f. 2.

Valves elongated, semi-lunate, suddenly attenuated into styliform beaks; strongly granulated transverse striæ, about 16 in .001″. Marine. (III. f. 20.)

Very likely this may prove to be *A. costata*; I give Ehrenberg's figure. This, and *A. Erebi*, named after the two vessels, Erebus and Terror, were from sea ice near north pole. In the *Mikrogeologie* the figures are wrongly referred to in the text.

VI.

57. *A. monilifera.* Greg. G. D. C., IV, f. 69.

Elliptic, slightly incurved at the apices, which forms short produced extremities; inner compartment obsolete, nodules on ventral margin; valves marked with longitudinal rows of distant round granules, giving a dotted aspect. Marine. Length .0017″ to .0026″, breadth .0008″ to .0011″. (III, f. 23.)

VII.

58. *A. aponina.* Kütz. K. B. T. V, f. 33.

Frustules small, hyaline, elliptic-lanceolate, constricted gently towards the ends; apices more or less produced or rostrate. Marine. Length .0009″ to .0019″, generally parasitic. (III. f. 22)

I have had it in great abundance, living two or three years after the original gathering was made, and had undergone the putrefac-

tive fermentation, the bottles meanwhile corked, and for months away from the light. Often the frustules cohered after self-division, forming in some cases, when viewed endwise, complete rings, after the manner of Ehrenberg's so called *Syncyclia*. The frustules were often hanging in festoons from the large, but now dead, frustules of *Melosira moniliformis*, of which the bulk of the gathering consisted.

A. veneta. Kütz. K. B. T. 3, f. 25.

Small, elliptic oblong, ends truncate, hyaline, dorsal surface of valves convex, venter straight; marine. Were it not for its fresh water habitat, *A. borealis* might also be united into this species.

59. *A. salina*. W. S. S. B. D., Pl. 30, f. 251.

Frustules elliptic oblong, with slightly produced truncate extremities, valves linear, rostrate, scarcely siliceous, finely transverse striate. Brackish. Length .0012″ to 0015″ striæ 64 in .001.″ (III, f. 29.)

There is great similarity between this and *A. aponina*, the latter, however, appears to be not only smaller, but more rigidly siliceous. Smith's figure shows it too coarsely striate.

60. *A. exigua*. Greg. G. D. C., T. IV, f. 75.

Linear elliptic, with somewhat obtuse ends. Marine. Length .0015″ to .0022″, striæ 28 in .001″. (III, f. 30.)

Rabenhorst considers this as *A. coffeæformis;* the longitudinal lines obsolete. I would unite with this,

A. macilenta. Greg. G. D. C., Pl. IV, f. 65.

Elliptic, long and narrow, contracting towards the ends, which are again slightly expanded. Median line well marked. Marine. Length .0018″ to .0022″, breadth .0005″ to .00086″, striæ about 30 in .001″.

61. *A. fluminensis*. Grun. 1863, T. XIII, f. 15.

Sub-orbicular, with obtusely truncate produced apices. Median lines of valves approximate, nearly straight. Marine. Length .0012″ to .0017″, breadth .0003″ to .0004″, striæ very fine, 50 in .001″. (III, f. 25.)

62. *A. Riechardtiana*. Grun. G. D. H. No figure.

Slightly bow-shaped, ends rounded, sometimes recurved, so that the appearance is very much like *Eunotia monodon;* inner margin of frustule curved, with line joining the ends and central nodule; transverse, radiate-punctate, striate, 30 to 40 in .001″.

63. *A. cymbifera.* Greg. G. D. C., Pl. VI, f. 97, 97*b*, 97*c*.
Broad, with short, produced, truncate apices; somewhat radiately coarsely striate, 22 in .001″. Marine. Length .0025″ to .0045″, breadth .0012″ to .0016″. (III, f. 26.)

64. *A. Ergadensis.* Greg. G. D. C., Pl. IV, f. 71.
Elliptic-lanceolate, narrow, with truncate apices, which are slightly expanded, nodules conspicuous, valves transversely striate. Marine. Length .0035″, breadth .00075″, striæ 24 in .001″ (III, f. 33.)

A. ventricosa. Greg. G. D. C., Pl. IV, f. 68.
This is figured as having straight median line, but, no doubt, belongs here. Length .0023″ to .0035″, breadth .0005″ to .0008″, striæ about 22 in .001″. A form to which Dr. Lewis first called my attention, and which Prof. A. M. Edwards provisionally named *lanceolata*, is common on the Atlantic coast; in *s. v.* it resembles, somewhat, *A. ventricosa;* it is very doubtfully an Amphora. (See No. 70.)

VIII.

65. *A. granulata.* Greg. G. D. C., Pl. V, f, 96, 96, *b. c. d. e. f.*
Linear, broad, with slightly convex sides, and truncate extremities; nodules inconspicuous, inner margins of valves and median line straight, or nearly so; dorsum marked with longitudinal convergent bars of sub-distant granules, 14 to 18 in .001″. Marine. Length .0017″ to .003″, breadth .0008″ to .0013″, valves transversely striate, 24 to 36 in .001″. (III, f. 31.)

66. *A. turgida.* Greg. G. D. C., Pl. IV, f. 63.
Nearly orbicular, with short, square, produced apices; nodules conspicuous. Marine. Length .001″ to .002″, breadth .0008″ to .0015″, striæ somewhat coarse, radiate, 24 in .001″. (III, f. 27.)

A. ventricosa. See *A. Ergadensis*, 64.

A. macilenta. See *A. exigua*, 60.

IX.

67. *A. borealis.* Kütz. K. B. T., III, f. 18.
Small, elliptic-lanceolate, sometimes acute, sometimes truncate. Fresh water. (III, f. 18.)

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A. affinis. Kütz., *non* W. S. K. B., T. 30, f. 66.
Oblong, apices broadly truncate, narrowed towards the ends, lon-
gitudinally striate, striæ of the middle very fine. Length .001″ to
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W. Smith's *A. affinis* may be Kützing's species, but it does not agree with it, either in figure or description. Smith's figure agrees exactly with specimens I obtained at Guernsey, and which I consider as *A. proboscidea*, Greg. They are slightly incurved on the outer margin; and the inner margins of the valves are bent outward at the ends of the frustule. The raphe is slightly inflexed; they have much the general aspect of *A. ovalis*.

X.

70. *A. lanceolata.* Cleve. C. S. D., T. 23, f. 2, *a*, *b*.

Front view lanceolate, ends somewhat constricted, striation distinct, slightly radiate, nodules indistinct, connecting zone without longitudinal lines, s. v. with bulging border, and extended rounded ends. Marine. Length .0052″, breadth .0016″, striæ nearly parallel, about 16 in .001″. (III, f. 34.)

Cleve remarks that this species is closely allied to *A. ventricosa*, Greg., from which it differs in the stronger striation, also in the elongated nodule on the ventral margin, probably, however, they are the same species. This appears to be the form already alluded to under *A. Ergadensis*, as common on the Atlantic coast. Prof. A. M. Edwards has recently informed me that I am probably correct in considering the species to which he had given the name "*lanceolata*" to be the same as Cleve's. I have already alluded to this form under No. 64. Gregory's figure of *A. ventricosa*, shows the ventral margin straight, while that of *A. Ergadensis* is apparently curved; a feature, after all, of little importance, though I have employed it for convenience in classifying.

cymbelloides. Grun. G. D. H. No figure.

Valves cymbellæform, oblong-lanceolate, ends truncate, equally lanceolate, somewhat pointed; dorsal margin very ., ventral curved slightly or not at all; median line straight, ..ral nodule small, valves very finely striate, striæ more conspicuous at the margin; dry frustules without color. Marine. Length .0014″ to .0031″, breadth .0004″ to .0005″, striæ about 80 in .001″.

Grunow remarks, "Not abundant; similar to Gregory's *angusta* and *nana* which have same breadth, and 40–50 striæ in .001″; perhaps it is *Syncyclia salpa, E.*" Var. *Mauritiana*, Grun., has valves more slender sub-acuminate, transverse striæ distinct, subradiant, longitudinal furrows more or less distinct in outer part of valves, which are pale yellow, striæ 65–70 in .001″.

72. *A. nana.* Greg. G. D. C., Pl. IV, f. 64.

Narrow, linear elliptic, sutural lines very near the ventral margin of valves, and nearly straight; nodules small near the ventral margin. Marine. Length .001″ to .0016″, breadth .0004″ to .0005″, striæ about 50 in .001″. (III, f. 32.)

73. *A. gracilis.* Ehr. E. M., Pl. 37, 3. f. 1.

Small, narrow, oblong, truncate; valves slender, transversely striate. (III, f. 35.)

Doubtful. To a slender variety of this, Ehrenberg gave the name *angusta.*

74. *A. marina.* W. S. A. N. H., 1857, Pl. I, f. 2.

Elliptic, with somewhat truncate extremities, nodules faint, striæ 40 in .001″. (III, f. 24.)

Smith says, "not unfrequent, but has been overlooked from its exact resemblance in outline to *A. affinis*, but may be known by its more delicate striæ, and inconspicuous nodules." If this is correct then Smith's figure, which is copied in Pl. III, f. 24, is poor indeed, for it has no resemblance at all to his *A. affinis*, and very little to Kützing's. Dr. Arnott, M. J. Vol. VI, p. 206, says: "*A. marina* of Smith, is precisely what Dr. Gregory calls *A. Proteus.* Some years ago Smith gave it the provisional name of *A. Scotica;* but omitted it in the second volume of his 'Snyopsis,' being not quite satisfied with its claims to be specifically distinguished from *A. affinis*, but these doubts were removed by afterwards finding it in

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Small, elliptic oblong, ends truncate, hyaline, dorsal surface of valves convex, venter straight; marine. Were it not for its fresh water habitat, *A. borealis* might also be united into this species.

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Small, elliptic-lanceolate, sometimes acute, sometimes truncate. Fresh water. (III, f. 18.)

A. veneta. See *A. aponina*, No. 58.

68. *A. crystallina.* Ehr. E. R. B. A., 1840. No figure.

Smooth, dorsal surface convex, venter concave; at each end broadly truncate, hyaline. Marine. (III, f. 37.)

I have specimens from Greenport, L. I., agreeing pretty well with the description; they are, however, pointed as well as truncate; indeed, often very much like W. Smith's figure of *A. hyalina*, without the longitudinal lines; perhaps it is only a variety of this latter.

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Oblong, apices broadly truncate, narrowed towards the ends, longitudinally striate, striæ of the middle very fine. Length .001″ to .002″. Marine. (III, f. 11.)

W. Smith's *A. affinis* may be Kützing's species, but it does not agree with it, either in figure or description. Smith's figure agrees exactly with specimens I obtained at Guernsey, and which I consider as *A. proboscidea*, Greg. They are slightly incurved on the outer margin; and the inner margins of the valves are bent outward at the ends of the frustule. The raphe is slightly inflexed; they have much the general aspect of *A. ovalis.*

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Small, valves cymbellæform, oblong-lanceolate, ends truncate, valves unequally lanceolate, somewhat pointed; dorsal margin very convex, ventral curved slightly or not at all; median line straight, central nodule small, valves very finely striate, striæ more conspicuous at the margin; dry frustules without color. Marine. Length .0014″ to .0031″, breadth .0004″ to .0005″, striæ about 80 in .001″.

Grunow remarks, "Not abundant; similar to Gregory's *angusta* and *nana* which have same breadth, and 40–50 striæ in .001″; perhaps it is *Syncyclia salpa, E.*" Var. *Mauritiana,* Grun., has valves more slender sub-acuminate, transverse striæ distinct, subradiant, longitudinal furrows more or less distinct in outer part of valves, which are pale yellow, striæ 65–70 in .001″.

72. *A. nana.* Greg. G. D. C., Pl. IV, f. 64.

Narrow, linear elliptic, sutural lines very near the ventral margin of valves, and nearly straight; nodules small near the ventral margin. Marine. Length .001″ to .0016″, breadth .0004″ to .0005″, striæ about 50 in .001″. (III, f. 32.)

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Small, narrow, oblong, truncate; valves slender, transversely striate. (III, f. 35.)

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XI.

35. *A. Semen.* Ehr. E. M., Pl. 38, 17, f. 10.

Inflated, with broad, shortly produced truncate ends, without striæ, (?). (II. f. 18.)

This is another of Ehrenberg's unsatisfactory species. The associated diatoms are fresh water forms, and the figure is very imperfect.

36. *A. pellucida.* Greg. G. D. C., Pl. IV, f. 73, 73, *b.*

Broad oval, delicate and transparent, nodules distinct, median line strongly inflexed, ventral margin excessively hyaline, outer portion of valves striate. Marine. Length .002″ to .003″, breadth .0012″ to .0018″, striæ shallow, 30 in .001. (II. f. 15.)

Rabenhorst considers this as a variety of *A. ovalis*, for which it might pass were it not for the marine habitat, and singular delicacy of the striæ; Ehrenberg's figure of *A. lineolata* from the Lüneburg deposit, and which is *A. ovalis*, is exceedingly like this.

A. incurva. Greg. M. J., III, Pl. IV, f. 6.

A. Erebi. Ehr. E. M., 35 A. 23, f. 3.

Single valves of these two species are figured; they appear to belong here. The description of *A. Erebi* is "lateral view arcuate, with obtuse apices, concave venter, and about 30 striæ in .001″." E. R. B. A. 1853, p. 526. The valve as figured, has a strongly arcuate raphe, exactly like the valves of *A. pellucida.*

37. *A. arenaria.* Donk. M. J., VI, Pl. III, f. 16.

Frustules hyaline, rectangular, colorless, extremities slightly rounded, sides somewhat uneven, slightly bulged out at the middle, and at the extremities. Dorsal surface faintly marked with six to eight longitudinal lines, the outer converging at the extremities; exceedingly transparent, and marked with transverse, very delicate moniliform striæ. Marine. Length .004″ to .006″, breadth about .0016″. (II. f. 14.)

38. *A. inflexa.* H. L. S. *Amphipleura inflexa.* Bréb. K. S. A.

Linear, valves arcuate, with rounded extremities; median lines distinct, nodules very minute, whole frustule hyaline. In f. v. median lines inflexed and touching the sutural lines; marine. (II. f. 16.)

There has been always some doubt of the propriety of placing this diatom among the *Amphipleuræ.* I received specimens from De Brébisson just before his death marked "not an *Amphipleura*; the

proper genus is *Toxonidia.*" Doubtless the inflexed median line induced him to place it in this genus. Grunow thinks it should be the type of a new genus.

XII.

39. *A. naviculacea.* Donk. M. J., Vol. I, N. S. Pl. I, f. 12.

Rectangular, extremities slightly rounded,, striæ on dorsal or outer half of the valve continuous, and nearly parallel; on the inner or ventral half coarser, and absent opposite the central nodule; strongly divergent on either side of it, and convergent near the terminal nodules; space between sutural lines blank. Marine. Length about .0032″, breadth .0011″. (III. f. 4.)

Donkin's figure in M. J. is not numbered on the plate.

40. *A. Donkinii.* Rab. F. E. A.=*A. lineolata.* Donk. M. J. I. N. S. Pl. I, f. 13.

Nearly rectangular, slightly convex laterally, hoop with several longitudinal plicæ, median line gently incurved. Marine. Length about .003″, breadth .0012″, finely striated transversely, striæ delicate. (III. f. 5.)

The name *lineolata* having already been appropriated by E. for another species of Amphora, Rabenhorst changed the name of this species to *Donkinii.* The figure is not numbered on the plate in M. J.

XIII.

41. *A. Proteus.* Greg. G. D. C., Pl. V, 81, 81*b*, 81*c*, 81*d*, 81*e*.

Somewhat variable in outline, lanceolate, elliptical, barrel-shaped, or truncate. Inner margin of raphe or median line, marked with longitudinal moniliform lines or bars. Marine. Length .0015″ to .006″, breadth .0013″ to .0024″, outer compartments distinctly striate, about 22 in .001″. (III. f. 1.)

The name indicates the variable appearance both as to size and outline of this form, which, were it not for the marine habitat, might pass for *A. ovalis.* It is exceedingly abundant on the Atlantic coast, and a variety from Florida, found also in New Jersey, shows a strong line limiting the striæ between the median line and the margin.

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Single valves of these two species are figured; they appear to belong here. The description of *A. Erebi* is "lateral view arcuate, with obtuse apices, concave venter, and about 30 striæ in .001″." E. R. B. A. 1853, p. 526. The valve as figured, has a strongly arcuate raphe, exactly like the valves of *A. pellucida.*

37. *A. arenaria.* Donk. M. J., VI, Pl. III, f. 16.

Frustules hyaline, rectangular, colorless, extremities slightly rounded, sides somewhat uneven, slightly bulged out at the middle, and at the extremities. Dorsal surface faintly marked with six to eight longitudinal lines, the outer converging at the extremities; exceedingly transparent, and marked with transverse, very delicate moniliform striæ. Marine. Length .004″ to .006″, breadth about .0016″. (II. f. 14.)

38. *A. inflexa.* H. L. S. *Amphipleura inflexa.* Bréb. K. S. A.

Linear, valves arcuate, with rounded extremities; median lines distinct, nodules very minute, whole frustule hyaline. In f. v. median lines inflexed and touching the sutural lines; marine. (II. f. 16.)

There has been always some doubt of the propriety of placing this diatom among the *Amphipleuræ.* I received specimens from De Brébisson just before his death marked "not an *Amphipleura*; the

proper genus is *Toxonidia.*" Doubtless the inflexed median line induced him to place it in this genus. Grunow thinks it should be the type of a new genus.

XII.

39. *A. naviculacea.* Donk. M. J., Vol. I, N. S. Pl. I, f. 12.

Rectangular, extremities slightly rounded,, striæ on dorsal or outer half of the valve continuous, and nearly parallel; on the inner or ventral half coarser, and absent opposite the central nodule; strongly divergent on either side of it, and convergent near the terminal nodules; space between sutural lines blank. Marine. Length about .0032″, breadth .0011″. (III. f. 4.)

Donkin's figure in M. J. is not numbered on the plate.

40. *A. Donkinii.* Rab. F. E. A.=*A. lineolata.* Donk. M. J. I. N. S. Pl. I, f. 13.

Nearly rectangular, slightly convex laterally, hoop with several longitudinal plicæ, median line gently incurved. Marine. Length about .003″, breadth .0012″, finely striated transversely, striæ delicate. (III. f. 5.)

The name *lineolata* having already been appropriated by E. for another species of Amphora, Rabenhorst changed the name of this species to *Donkinii.* The figure is not numbered on the plate in M. J.

XIII.

41. *A. Proteus.* Greg. G. D. C., Pl. V, 81, 81*b*, 81*c*, 81*d*, 81*e*.

Somewhat variable in outline, lanceolate, elliptical, barrel-shaped, or truncate. Inner margin of raphe or median line, marked with longitudinal moniliform lines or bars. Marine. Length .0015″ to .006″, breadth .0013″ to .0024″, outer compartments distinctly striate, about 22 in .001″. (III. f. 1.)

The name indicates the variable appearance both as to size and outline of this form, which, were it not for the marine habitat, might pass for *A. ovalis.* It is exceedingly abundant on the Atlantic coast, and a variety from Florida, found also in New Jersey, shows a strong line limiting the striæ between the median line and the margin.

striate. Marine. Length .0035″ to .0055″, breadth .00075″, striæ about 36 in .001″. (III, f. 14.)

Gregory gives no front view of this species, but remarks that probably it is like *A. nobilis*, from which it differs by the inner margin being straight, and valves stronger moniliform striate; it is quite probable they may be varieties of same species.

52. *A. rectangularis.* Greg. T. M., v, Pl. 1, f. 29.

Nearly rectangular, slightly constricted, ends rounded, their margins slightly undulate, hyaline part of connecting zone widest at the ends, length .0025″ to .0045″, valves transversely striate, 40 in .001″. (III. f. 13.)

This is doubtfully an Amphora; Prof. Gregory placed it here on account of the stauros; probably it is a (*Navicula*) *Stauroneis.* I have found it associated with *A. plicata* in gatherings from Atlantic City.

v.

53. *A. coffeæformis.* Kütz. K. B., Pl. v, f. 36.

Elliptic oblong or lanceolate, turgid at the middle, apices somewhat elongated, truncate; longitudinal lines on the marginal, and very fine ones on the central, portion; length .0012″ to .0021″. (III. f. 17.)

A. lineolata. Kütz. K. B., Pl. 5, f. 36. (III. f. 16.)

A. Fischerii. Kütz. K. B., Pl. 5, f. 38. (III. f. 19.)

It is already conceded by Ralfs, Rabenhorst, and others, that *A. lineolata* of Kützing is not *A. lineolata* of E., and that *A. Fischerii* should be united with *A. coffeæformis. A. Hohenackeri*, and *A. quadricostata* differ in having a straight median line, and apices not produced, yet little reliance can be placed on the figures, and they may prove to belong here. Rabenhorst gives also *A. acutiuscula* as a synonym; it appears, however, to be a Navicula. He also quotes as synonym *A. exigua*, Greg., which is much more coarsely striate, 28 in .001″, and also *A. lineata*, Greg., which I have considered an entirely distinct form.

54. *A. lineata.* Greg. G. D. C., Pl. IV, f. 70.

Elliptic or elliptic-lanceolate, with short produced apices, which are truncate; outer portion of valves marked with strong longitudinal lines. The whole frustule has a characteristic linear aspect.

Marine. Length .0022″ to .003″, breadth .0007″ to .008″, finely striate, 42 in .001. (III. f. 21.)

I am by no means sure that Rabenhorst is not right in uniting this with *A. coffeæformis*; it is closely allied to the next species, and with it is not uncommon in gatherings from the Atlantic coast.

55. *A. costata.* W. S. S. B. D., Pl. xxx, f. 253.

Frustules ventricose, with short, broad, truncate beaks, longitudinally costate; transversely striate, 16 in .001″, according to Prof. Gregory; my own measures give 24 in .001″, and Smith's figure appears altogether too coarse. I found it in brackish ponds, N. Haven. Marine, or brackish. Length .002″ to .0033″, breadth .0012″ to .0016″. W. Smith's figure. (III. f. 28.)

56. *A. Terroris.* E. E.R.B.A., 1853, p. 156. E. M., 35 A. 23, f. 2.

Valves elongated, semi-lunate, suddenly attenuated into styliform beaks; strongly granulated transverse striæ, about 16 in .001″. Marine. (III. f. 20.)

Very likely this may prove to be *A. costata*; I give Ehrenberg's figure. This, and *A. Erebi*, named after the two vessels, Erebus and Terror, were from sea ice near north pole. In the *Mikrogeologie* the figures are wrongly referred to in the text.

VI.

57. *A. monilifera.* Greg. G. D. C., IV, f. 69.

Elliptic, slightly incurved at the apices, which forms short produced extremities; inner compartment obsolete, nodules on ventral margin; valves marked with longitudinal rows of distant round granules, giving a dotted aspect. Marine. Length .0017″ to .0026″, breadth .0008″ to .0011″. (III, f. 23.)

VII.

58. *A. aponina.* Kütz. K. B. T. v, f. 33.

Frustules small, hyaline, elliptic-lanceolate, constricted gently towards the ends; apices more or less produced or rostrate. Marine. Length .0009″ to .0019″, generally parasitic. (III. f. 22)

I have had it in great abundance, living two or three years after the original gathering was made, and had undergone the putrefac-

tive fermentation, the bottles meanwhile corked, and for months away from the light. Often the frustules cohered after self-division, forming in some cases, when viewed endwise, complete rings, after the manner of Ehrenberg's so called *Syncyclia*. The frustules were often hanging in festoons from the large, but now dead, frustules of *Melosira moniliformis*, of which the bulk of the gathering consisted.

A. veneta. Kütz. K. B. T. 3, f. 25.

Small, elliptic oblong, ends truncate, hyaline, dorsal surface of valves convex, venter straight; marine. Were it not for its fresh water habitat, *A. borealis* might also be united into this species.

59. *A. salina*. W. S. S. B. D., Pl. 30, f. 251.

Frustules elliptic oblong, with slightly produced truncate extremities, valves linear, rostrate, scarcely siliceous, finely transverse striate. Brackish. Length .0012″ to 0015″ striæ 64 in .001.″ (III, f. 29.)

There is great similarity between this and *A. aponina*, the latter, however, appears to be not only smaller, but more rigidly siliceous. Smith's figure shows it too coarsely striate.

60. *A. exigua*. Greg. G. D. C., T. IV, f. 75.

Linear elliptic, with somewhat obtuse ends. Marine. Length .0015″ to .0022″, striæ 28 in .001″. (III, f. 30.)

Rabenhorst considers this as *A. coffeæformis;* the longitudinal lines obsolete. I would unite with this,

A. macilenta. Greg. G. D. C., Pl. IV, f. 65.

Elliptic, long and narrow, contracting towards the ends, which are again slightly expanded. Median line well marked. Marine. Length .0018″ to .0022″, breadth .0005″ to .00086″, striæ about 30 in .001″.

61. *A. fluminensis*. Grun. 1863, T. XIII, f. 15.

Sub-orbicular, with obtusely truncate produced apices. Median lines of valves approximate, nearly straight. Marine. Length .0012″ to .0017″, breadth .0003″ to .0004″, striæ very fine, 50 in .001″. (III, f. 25.)

62. *A. Riechardtiana*. Grun. G. D. H. No figure.

Slightly bow-shaped, ends rounded, sometimes recurved, so that the appearance is very much like *Eunotia monodon;* inner margin of frustule curved, with line joining the ends and central nodule; transverse, radiate-punctate, striate, 30 to 40 in .001″.

63. *A. cymbifera.* Greg. G. D. C., Pl. VI, f. 97, 97*b*, 97*c*.
Broad, with short, produced, truncate apices; somewhat radiately coarsely striate, 22 in .001″. Marine. Length .0025″ to .0045″, breadth .0012″ to .0016″. (III, f. 26.)

64. *A. Ergadensis.* Greg. G. D. C., Pl. IV, f. 71.
Elliptic-lanceolate, narrow, with truncate apices, which are slightly expanded, nodules conspicuous, valves transversely striate. Marine. Length .0035″, breadth .00075″, striæ 24 in .001″ (III, f. 33.)

A. ventricosa. Greg. G. D. C., Pl. IV, f. 68.
This is figured as having straight median line, but, no doubt, belongs here. Length .0023″ to .0035″, breadth .0005″ to .0008″, striæ about 22 in .001″. A form to which Dr. Lewis first called my attention, and which Prof. A. M. Edwards provisionally named *lanceolata*, is common on the Atlantic coast; in *s. v.* it resembles, somewhat, *A. ventricosa;* it is very doubtfully an Amphora. (See No. 70.)

VIII.

65. *A. granulata.* Greg. G. D. C., Pl. V, f, 96, 96, *b. c. d. e. f.*
Linear, broad, with slightly convex sides, and truncate extremities; nodules inconspicuous, inner margins of valves and median line straight, or nearly so; dorsum marked with longitudinal convergent bars of sub-distant granules, 14 to 18 in .001″. Marine. Length .0017″ to .003″, breadth .0008″ to .0013″, valves transversely striate, 24 to 36 in .001″. (III, f. 31.)

66. *A. turgida.* Greg. G. D. C., Pl. IV, f. 63.
Nearly orbicular, with short, square, produced apices; nodules conspicuous. Marine. Length .001″ to .002″, breadth .0008″ to .0015″, striæ somewhat coarse, radiate, 24 in .001″. (III, f. 27.)

A. ventricosa. See *A. Ergadensis*, 64.

A. macilenta. See *A. exigua*, 60.

IX.

67. *A. borealis.* Kütz. K. B. T., III, f. 18.
Small, elliptic-lanceolate, sometimes acute, sometimes truncate. Fresh water. (III, f. 18.)

A. veneta. See *A. aponina*, No. 58.

68. *A. crystallina.* Ehr. E. R. B. A., 1840. No figure.
Smooth, dorsal surface convex, venter concave; at each end broadly truncate, hyaline. Marine. (III, f. 37.)
I have specimens from Greenport, L. I., agreeing pretty well with the description; they are, however, pointed as well as truncate; indeed, often very much like W. Smith's figure of *A. hyalina*, without the longitudinal lines; perhaps it is only a variety of this latter.

69. *A. affinis.* Kütz., *non* W. S. K. B., T. 30, f. 66.
Oblong, apices broadly truncate, narrowed towards the ends, longitudinally striate, striæ of the middle very fine. Length .001″ to .002″. Marine. (III, f. 11.)
W. Smith's *A. affinis* may be Kützing's species, but it does not agree with it, either in figure or description. Smith's figure agrees exactly with specimens I obtained at Guernsey, and which I consider as *A. proboscidea*, Greg. They are slightly incurved on the outer margin; and the inner margins of the valves are bent outward at the ends of the frustule. The raphe is slightly inflexed; they have much the general aspect of *A. ovalis*.

X.

70. *A. lanceolata.* Cleve. C. S. D., T. 23, f. 2, *a*, *b*.
Front view lanceolate, ends somewhat constricted, striation distinct, slightly radiate, nodules indistinct, connecting zone without longitudinal lines, s. v. with bulging border, and extended rounded ends. Marine. Length .0052″, breadth .0016″, striæ nearly parallel, about 16 in .001″. (III, f. 34.)

Cleve remarks that this species is closely allied to *A. ventricosa*, Greg., from which it differs in the stronger striation, also in the elongated nodule on the ventral margin, probably, however, they are the same species. This appears to be the form already alluded to under *A. Ergadensis*, as common on the Atlantic coast. Prof. A. M. Edwards has recently informed me that I am probably correct in considering the species to which he had given the name "*lanceolata*" to be the same as Cleve's. I have already alluded to this form under No. 64. Gregory's figure of *A. ventricosa*, shows the ventral margin straight, while that of *A. Ergadensis* is apparently curved; a feature, after all, of little importance, though I have employed it for convenience in classifying.

71. *A. cymbelloides.* Grun. G. D. H. No figure.

Small, valves cymbellæform, oblong-lanceolate, ends truncate, valves unequally lanceolate, somewhat pointed; dorsal margin very convex, ventral curved slightly or not at all; median line straight, central nodule small, valves very finely striate, striæ more conspicuous at the margin; dry frustules without color. Marine. Length .0014″ to .0031″, breadth .0004″ to .0005″, striæ about 80 in .001″.

Grunow remarks, "Not abundant; similar to Gregory's *angusta* and *nana* which have same breadth, and 40–50 striæ in .001″; perhaps it is *Syncyclia salpa, E.*" Var. *Mauritiana*, Grun., has valves more slender sub-acuminate, transverse striæ distinct, subradiant, longitudinal furrows more or less distinct in outer part of valves, which are pale yellow, striæ 65–70 in .001″.

72. *A. nana.* Greg. G. D. C., Pl. IV, f. 64.

Narrow, linear elliptic, sutural lines very near the ventral margin of valves, and nearly straight; nodules small near the ventral margin. Marine. Length .001″ to .0016″, breadth .0004″ to .0005″, striæ about 50 in .001″. (III, f. 32.)

73. *A. gracilis.* Ehr. E. M., Pl. 37, 3. f. 1.

Small, narrow, oblong, truncate; valves slender, transversely striate. (III, f. 35.)

Doubtful. To a slender variety of this, Ehrenberg gave the name *angusta.*

74. *A. marina.* W. S. A. N. H., 1857, Pl. I, f. 2.

Elliptic, with somewhat truncate extremities, nodules faint, striæ 40 in .001″. (III, f. 24.)

Smith says, "not unfrequent, but has been overlooked from its exact resemblance in outline to *A. affinis*, but may be known by its more delicate striæ, and inconspicuous nodules." If this is correct then Smith's figure, which is copied in Pl. III, f. 24, is poor indeed, for it has no resemblance at all to his *A. affinis*, and very little to Kützing's. Dr. Arnott, M. J. Vol. VI, p. 206, says: "*A. marina* of Smith, is precisely what Dr. Gregory calls *A. Proteus.* Some years ago Smith gave it the provisional name of *A. Scotica;* but omitted it in the second volume of his 'Snyopsis,' being not quite satisfied with its claims to be specifically distinguished from *A. affinis*, but these doubts were removed by afterwards finding it in

TABLE II.—GRADE OF PENETRATION.

Objective.		Central Light.	Oblique Light.
Gundlach	I	II	
Nachet	O	II	
"	I	IV	
Gundlach	II	V	
"	III	VI	
"	IV	VI	
Hartnack	IV	VI	
Nachet	V	VIII	XII
Hartnack	VII	VIII	XII
Gundlach	V	X	XII
"	VI	X	XII
Hartnack	VIII	X	XII
Zeiss	F	X	XII
Hartnack	IX	XII	XIII
"	X	XII	XVI
Powell & Lealand (*immersion*)	$\frac{1}{12}$	XII	XVI
Gundlach	VII	XII	XVI
"	VIII	XII	XVI
"	IX	XII	XVI
"	X	XII	XVI

TRICERATIUM FIMBRIATUM?

In the number of the LENS for April, 1872, Vol. I, page 100, is a paper by Dr. Woodward, on the double markings of Triceratium, wherein he figures two valves, one whole, the other broken, as, both of them, belonging to *Triceratium Fimbriatum.* If my friend Dr. Woodward, will permit me to do so, I should like to say something about his plate and the species.

First, then, as to the species. It was founded on what is now generally considered very insufficient grounds by Dr. G. C. Wallich, in 1858 (*Quar. Jour. Mic. Sci.*, VI. page 242), and Ralfs has (*Prit.*

Infus. 1861, page 855), ranked it under the older name of *T. Favus*. For my part, although I have never seen Dr. Wallich's original specimens, I must say I think it cannot be separated from that species. Möller in his Typen-Platte, has chosen to retain the name, attaching it to a four-sided form and giving Brightwell as the founder. I do not wish to be too severe on Mr. Möller, who has given us such beautiful specimens of his mechanical skill, but I have known of more than one beginner at the Diatoms led astray by errors which have crept into his slides. The form he names *T. fimbriatum*, can not be, with justice, separated specifically from *T. Favus*, Ehr., as Dr. Woodward's plate shows plainly. The finer set of markings can be shown in every valve of *T. Favus* which has not been too long acted upon by chemicals. As to the other specimen figured in the plate, and which is in the cabinet of Dr. Johnston, I have seen and examined it critically. Dr. Johnston lent me the specimen in 1866, and I took several photographs of it. I was particularly interested in it as it came from the Moron earth, and I had found the same species some time before in the Monterey deposit, but with six sides. About the same time Mr. C. G. Bush, of Boston, found a three-sided form of the same in the Monterey material and sent it to me for photographing. I obtained one or two pretty good negatives of it and sent it back to him. Soon after I was sorry to hear that the balsam had contracted, drawing the cover down and breaking the diatom. I have never been able to find another three-sided form of this, as I consider it, distinct species. I also lost my six-sided form, and for awhile was in despair. Thereafter, however, I found in the Monterey material a beautiful and perfect six-sided valve, besides several fragments. The group including Dr. Johnston's, Mr. Bush's and my specimens, I consider deserves to rank as a separate species, and I have provisionally, in the manuscript of my report on the specimens collected by the California State Survey, called it *Triceratium ponderosum*. Therefore, I would ask as a favor of Diatomists, that, until my said report sees the light, when I will give my reasons for so ranking these forms, they be called by the name I have proposed for them.

A. Mead Edwards, M. D.

Newark, N. J.

THE FIGURE OF THE EARTH; AND ITS EFFECT ON ASTRONOMICAL OBSERVATIONS MADE IN THE PLANE OF THE MERIDIAN.

It is now exactly two centuries since the deviation of the earth from the strictly spherical form was first noticed; when Picard found that the pendulum of his transit clock, which beat seconds at Paris (France), must be shortened to beat seconds at Cayenne, near the Equator. The fact that the earth is flattened at the poles, giving an elliptic shape to any meridian coinciding with the sea level, was demonstrated by Sir Isaac Newton, about the same time, to be a necessary consequence of rotation on the axis; and it was practically proved by measurements of different parts of the earth's surface, made in the last century. From this followed the important astronomical fact that the perpendicular, or line pointing to the zenith, does not coincide with the line directed from the earth's centre through the place of observation, except at the equator and the poles.

There were, however, numerous discrepancies between the results thus obtained, which could not be reconciled with the theory that the ocean level forms the surface of a simple spheroid; though those results were discussed by several of the ablest mathematicians of the time. Within a few years past, the problem has been again attacked; and it is now claimed to be established that the equatorial curve itself at the sea level is an ellipse, having a major axis 8800 feet, or 1 2-3 miles longer than the minor axis. The following table shows the differences between the measures recently deduced and those given by Bessel.

	True: Feet.	*Bessel: Feet.*
Major equatorial radius	20,926,400	20,923,600
Minor equatorial radius	20,922,000	20,923,600
Mean equatorial radius	20,924,200	20,923,600
Polar radius	20,854,350	20,853,660

These give the following results for the Chicago Observatory, (north lat. 41° 50′ 1″; long. west, 0 h. 42m. 14.72s.; or 10° 33′ 40.8″), assuming the ground surface to be 590 feet above the sea level:

Distance from earth's center, feet	20,892,740
Proportion of same to mean eq. radius	0,998,497
Logarithm of which is	9.9993467
One degree of latitude, feet	364,380.3
Difference for 10 minutes of arc (plus)	10.58
One degree of longitude, feet	272,479
Difference for 10 minutes of arc (minus)	706.93
Angle of Vertical	0° 11′ 5.53″
Angle by Bessel's table	11′ 26.18″

The new mean equatorial radius is 600 feet greater than the old value; yet the distance of Chicago from the earth's centre is above 400 feet less than the value of that quantity as calculated on the supposition that the earth's equator is a circle, with a radius of 20,923,600 feet.

This little difference of (say) 1000 feet is, however, insignificant as compared with that apparent when we consider the effect on longitude. The position of the major axis of the equatorial ellipse is 14° 23′ east from Greenwich, and 165° 37′ west from that meridian. Hence the observatory of Chicago is situated 12° west from the meridian, the plane of which passes through the minor axis. Now, a little calculation shows us that at the corresponding point on the equator the perpendicular to the tangent line inclines 0′ 17.65″ eastward from a line directed from the earth's centre. In other words the geographical meridian of observation makes so much of an angle with the true meridian, the plane of which passes through the earth's centre.

Multiplying 17.65″ into the cosine of the latitude, we obtain 13.15″ as the deviation of the perpendicular at the Chicago Observatory from the plane of the true meridian. And we arrive at the startling conclusion that a correction must be made for this hitherto unacknowledged error of direction, in every observation in right ascension made with the Dearborn transit instrument, if we would ascertain the exact positions of the objects observed.

It is true that this discrepancy is not so great as it may appear at the first blush. The transits of all stars being observed on the same false meridian, those stars lying on the same parallels of declination will exhibit (sensibly) the same differences of right ascension as if observed at their transits over the true meridian. But if two stars

have a considerable difference of declination, especially if on the same side of the zenith, the error will be an important one. The stars catalogued at the Dearborn Observatory are all "zenith stars"; and at 5° of zenith distance the *change* in the direction of the false meridian is but 0.07 seconds of arc, or one out of 200 equal parts of a second of time; so that the errors in their *differences* of right ascension will be very small. But it is important to note that the *constant* of their errors in right ascension will be large if the fundamental stars do not also culminate near the zenith; and the places of all the fundamental stars should be carefully revised from observations made only at observatories noted below as being but little affected by this error. The Greenwich observations should not be used in this revision.

It is evident that as the different observatories on the earth's surface are widely scattered in longitude, the error of one, due to this deviation from the perpendicular, will be different in amount (sometimes in direction) from the errors of the rest. And here we have a fact, which, if rightly applied, will enable practical astronomers to enter on a series of comparative observations for the purpose of ascertaining with a little more precision, the shape of the earth's surface, and the errors of observation resulting therefrom. It is very probable that in the ellipticity of the earth's equator, we have the true cause of several of the difficulties that have been met with in reconciling the positions of stars as taken at different observatories. I use the word "stars" in its more extended sense; for I cannot resist the hope that many, if not all, of the snags that are perpetually encountered in the attempt to make the calculated lunar elements agree with her observed places, will be traced to the fact that our observations are not made in the plane of the true meridian.

It is singular, however, that while Chicago and Greenwich are so situated that observations made at those places are chargeable with large errors on this account, there are a great many observatories, including the national one at Washington, which lie almost exactly on one of the meridians that pass through the axes of the equatorial ellipse. The following table shows the distances of some of these from the major axis, and the error of the vertical *at the equator*.

These equatorial errors must be multiplied into the cosine of the latitude to obtain the deviation at the observatory.

Observatory.	*Longitude.*	*Angle Secs.*
Prague	0° 2½ E	0.06
Naples	0° 8 W	0.22
Kremsmunster	0° 15 W	0.39
Philadelphia (High School)	89° 32½ W	0.72
Berlin	0° 59 W	1.54
Palermo	1° 2 W	1.62
Washington	91° 26 W	2.24
Copenhagen	1° 48 W	2.84
Romé	1° 54 W	2.98
Leipsic	1° 59 W	3.07
Vienna	2° 0 E	3.11
Padua	2° 31 W	3.92
Munich	2° 46½ W	4.35
Cape Good Hope	4° 6 E	6.40
Cambridge (U. S.)	85° 30½ W	7.02
Chicago	102° 0 W	17.65
Greenwich	14° 23 W	21.64

It would, perhaps, scarcely be advisable as yet, to calculate and apply a table of corrections, for this meridional error at each observatory; because we are not absolutely certain that the exact amount and direction of the equatorial ellipticity has been determined. We do know, however, that the assumptions above stated harmonize with the measured facts much more closely than the circular theory; and it would, therefore, be advisable that our celestial land-marks be accepted only as they have been determined at places situated near the prime meridians, and that we should choose as fundamentals only the stars that differ but little in declination from those which are to be observed. The latter condition is generally adhered to; but for other reasons.

The proposed observatory on the Rocky Mountains will be unfavorably situated in this respect; though affording unequaled facilities for the discovery of new objects, and the close examination of old ones. The Australian observatory is nearly midway between the major and minor meridians; and its "work" in cataloguing the stars in the southern hemisphere will, therefore, be charged with more error than that of the observatory recently established in South America.

This matter has an important bearing upon another grand problem—the distance of the sun from the earth—which, it is hoped, will be solved from the observations of the transit of Venus, in December, 1874. If the earth's equator be an ellipse, then the length of a degree on any given parallel of latitude, varies with the longitude: and a degree of the meridian is longer at Washington than on the same latitude in Greece. Hence the lengths of the base lines connecting the several stations from which the transit will be observed, are not the same as if the earth were a true oblate spheroid at the sea level. An error of one mile in the calculation of these base lines would involve an error of 20,000 miles in the computation of distance from earth to sun, though the measures of *position* were absolutely exact.

The above considerations suggest the advisability of remodeling our tables of longitude. Hitherto it has been said that we have no natural starting point; and each nation has been free to reckon longitudes from its own capital, or national observatory—as some date their acts of legislation from the accession of their rulers. Our prime meridian of longitude runs through the Mediteranean, Mount Vesuvius, and Prague; and within one degree (53′) of Uraniburg, the meridian to which the Rudolphine tables were calculated by the immortal Kepler from the observations of Tycho Brahe. We have just as much reason for reckoning our longitudes from that meridian as we have for reckoning latitude from the equator; and the change, so obviously proper in itself, would be attended with very little trouble.

We must not forget that, even with the correction noted above, the discrepancies found in dealing with the earth's figure do not entirely disappear; but they are very much reduced in number and amount: and most of those remaining could perhaps be accounted for if we knew the densities of the underlying strata at the several places where the pendulum experiments have been made for determining the force of the attraction of gravity. It is well known that the earth's crust is not homogeneous. With regard to the condition of its interior we may remark that while the experiments of Maskelyne, Cavendish, and others, prove that the average density (5.4) of the whole mass is at least double that of the crust, yet there is reason to believe that the density near the centre is not

so enormous as some have supposed must result from the exterior pressure. It has been demonstrated that the excess of the equatorial over the polar radius, divided by the latter, would be one in 230, if the earth were homogeneous throughout, all the particles attracting each other; while it would be one in 580 if the force of attraction acted solely at the centre of the mass. Our measures give nearly one in 289.44, and one in 308.27, for the two equatorial axes; average one in 298.6, showing a compression at the centre which is comparable with the density at the surface.

In conclusion, I will state my suspicion that the above quoted idea of what we may call a double ellipticity, is but an approximation to the truth. The major meridian passes through the continents of the old world; and, on the other side of the globe, it traverses the whole extent of a vast ocean—the Pacific. Now, we cannot conceive of any other cause for departure from the circle, in a figure of revolution, than the necessity of preserving an equilibrium, among materials of different attractive force (weight). In the ocean of water on the Pacific side of the globe, we have a mass of matter that is only 0.4 the density of the old world continents, for a depth of perhaps several miles. It is difficult to see that the equilibrium could be preserved unless the bulk of matter on the Pacific side of the earth's axis, were increased in inverse proportion to its relative density; and further search will probably lead to the conclusion that, while the parallels of latitude on the continent side are nearly circular, those on the Pacific side form semi-ellipses, the eccentricity of which is somewhat more than twice as great as that now ascribed to the equator and the parallels to that curve.

This subject has an even more important bearing on the determination of the unit of length in the solar system; as the observations of the next transit of Venus will necessarily be made from the Pacific side of our globe.

E. Colbert.

Chicago.

have a considerable difference of declination, especially if on the same side of the zenith, the error will be an important one. The stars catalogued at the Dearborn Observatory are all "zenith stars"; and at 5° of zenith distance the *change* in the direction of the false meridian is but 0.07 seconds of arc, or one out of 200 equal parts of a second of time; so that the errors in their *differences* of right ascension will be very small. But it is important to note that the *constant* of their errors in right ascension will be large if the fundamental stars do not also culminate near the zenith; and the places of all the fundamental stars should be carefully revised from observations made only at observatories noted below as being but little affected by this error. The Greenwich observations should not be used in this revision.

It is evident that as the different observatories on the earth's surface are widely scattered in longitude, the error of one, due to this deviation from the perpendicular, will be different in amount (sometimes in direction) from the errors of the rest. And here we have a fact, which, if rightly applied, will enable practical astronomers to enter on a series of comparative observations for the purpose of ascertaining with a little more precision, the shape of the earth's surface, and the errors of observation resulting therefrom. It is very probable that in the ellipticity of the earth's equator, we have the true cause of several of the difficulties that have been met with in reconciling the positions of stars as taken at different observatories. I use the word "stars" in its more extended sense; for I cannot resist the hope that many, if not all, of the snags that are perpetually encountered in the attempt to make the calculated lunar elements agree with her observed places, will be traced to the fact that our observations are not made in the plane of the true meridian.

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DISTINGUISHING FIBRES IN MIXED GOODS.—*Mr. Charles Stodder*, the veteran Boston microscopist, in a recent issue of the *Scientific American*, thus answers the enquiry of a correspondent: "Unquestionably the microscope is the best means of accomplishing the purpose of your correspondent; it is the simplest, quickest, easiest and surest. All and each of the fibres named in the article are constructed—built up, so to speak—in different manners, so distinct from each other that a moderate magnifying power, say 400 diameters, of a decently good instrument, will show at once what they are. Any one with a very little skill in manipulation can obtain the result. The differences have been described and figured in the books, but there is no need of books. Every one can obtain genuine fibres of either kind, with almost less trouble than referring to a book, for comparison with those found in the fabric, and the original comparison is of far more value than the authority of a picture. No chemical test is known to distinguish flax from cotton fibre, but their difference in the microscope may be seen at a glance. Jute fibre has more resemblance to flax, but can be distinguished with a little more study. The materials of paper may also be ascertained, in part at least, by the microscope: for example, your number dated March 15, is printed on paper containing no cotton or linen; it is mostly wood fibre, with "pitted" and "scalariform" ducts, not peculiar to any kind of wood, with possibly fibres of manilla, esparto or ramie, of which I have not the means of comparison.

"But the microscope cannot do everything. There is a certain fabric in use purporting to be made entirely of cows' hair. The question came up: Is there any sheep's wool in it? This could not be answered. For, while the bulk of each is easily distinguished, there are some hairs from each animal that cannot be known from the other. In this case, so far as is known, chemistry is equally powerless."

LENS FIRES.—*Dr. H. C. Bolton*, of Columbia College, New York City, states that on a recent occasion, at 9 A. M., on entering his laboratory he found a wooden table on fire, ignition having been occasioned by the rays of the morning sun, which fell upon a glass spherical flask containing water. The flask served as a lens, which concentrated the rays and set fire to the wood. Dr. Bolton also alludes to the statement of Lactantius (A. D. 300), who mentions the use of glass globes, filled with water, to be used in kindling fires; while Pliny recommends the use of lenses for the purpose of cauterizing the flesh of sick persons. As to the latter, one Mr. Barnes, of Connecticut, took a patent in this country some five years ago, for the use of lenses for the purpose suggested by Pliny.

In respect to fires occasioned by lenses, doubtless there are many examples. It is well known that vessels at sea have been set on fire by the bulls-eye glasses used to admit light to between decks. These glasses were formerly made convex on one side, thus forming powerful lenses. In consequence of the loss of property and danger, their use has been discontinued, and thick plates of glass, flat on both sides, have been generally substituted. Captain Scoresby and Dr. Kane used to astonish the natives of the polar regions by taking blocks of clear ice and cutting them into the form of lenses, with which they instantly kindled fires.

BRAIN STIMULANTS.—On the subject of Brain Stimulants, we find in a recent number of *Hygiene* the following sensible suggestions:

A prominent clergyman in a neighboring city writes us, that for many years he has been in the habit of limiting his use of tea and coffee, and his "occasional cigar," to the latter part of the week, and, as he fancies, with the result of being able to compose with less effort than when he has either abstained entirely from their use, or when, as once or twice, he has indulged in them continuously for a brief period.

Herein is a valuable suggestion to brain-workers in any profession the exigencies of which call for occasionally increased and severe efforts. Tea, coffee, tobacco and alcohol, by retarding the changes in the tissues of the body, which is their physiological action, are supposed to allow the energy thus conserved to manifest itself in the higher form of cerebral activity—in simple language, they are stimulants to the nervous system; and, in the proper dose, there can be no question that they do exalt and stimulate brain-action. But there is equally no question that the retarded tissue changes are at the expense of vitality generally —the vitality of the body, that is, its health and strength, being ever in relation to the newness of the atoms which compose the body—and these tissue-changes, the work of waste and repair, must be accelerated in some manner, and to a corresponding extent, in order to preserve the balance.

The obvious lesson to be gained from these facts, is, that during periods of intense and unusual mental activity—a lawyer in trying an engrossing case, a banker during a financial stress, a company officer at periods of increased responsibility, an editor or political leader carrying through an important measure—that at such times brain-work may be done with more facility and at less expense, by a judicious use of this class of agents. Provided, however—provided that the balance be struck at once when the necessity for them is obviated.

The means of restoring the balance include first, abstinence from the agents themselves; second, comparative rest for the brain; and lastly, and quite as important as the preceding, those measures which accelerate tissue-changes and of which the essential ones are physical exercise and bathing—notably, the Turkish bath—and nutritious, easily assimilated food, by the first of which, the breaking down of the older particles, and the excretion of poisonous waste-matter are facilitated, and so tissue-change in the interest of waste is promoted; while the last furnishes material for renewal and growth.

With such a regimen, based on an intelligent application of means to ends, we would have a fewer cases of men prematurely breaking down under efforts they might make with ease did they only know when and how to open the throttle-valve, or to put on the brakes. This view of the subject must not be construed into an argument for a mere sensual indulgence. It is intended for men as they are, and with regard to conditions as they exist. These agents are used, and probably always will be. They have their uses; and knowledge of these will do more to prevent their abuse than the wholesale condemnation which frequently arises from ignorance.

IRRITABILITY OF THE FROG'S HEART.—Taken all in all, the batrachians are marvelous beings. Besides being obliged to pump air down into their own lungs, which explains why the gular membrane underneath the under jaw is so elastic, acting on the volume of inhaled air in the cavity of the mouth on the mechanical principle of bellows, they catch game with the point of the tongue, drink through the spongy texture of the skin on the back, and live months in succession concealed in the mud bed of a pool without respiring; and yet the systole and diastole, or in plainer words, the contraction and expansion of the heart, is not suspended. Their vitality is remarkable, since the small amount of oxygen introduced into the arterial blood when making the final plunge in autumn keeps the spark of life alive till emerging from the water in the spring. If the heart of a frog is cut from its connections within the pericardium and placed on a table, it will pulsate and throb energetically for some minutes. When apparently quiescent, the point of a needle will rouse it again into spasmodic energy. Finally, by the touch of irritants, its irritability is completely exhausted. After experimenting full half an hour in that manner, we were struck with the lively vaultings of the frog from which the heart had been taken. Certainly it was conscious of its relations, for it avoided many cautious attempts to capture it, on the part of the operator. It was some hours before death closed the scene.

The vital tenacity of reptiles, particularly batrachians and chelonians, which include the tortoise family, are remarkable, and worthy of more extended scientific investigation.

AN INLAND SEA THAT NEVER GIVES UP ITS DEAD.—Some twelve or fourteen persons have been drowned in Lake Tahoe, California, within the past ten years; none of the bodies have ever been recovered. Superstition, ever ready to weave a sensation from nature's laws, asserts that there was a doubtful mystery in the non-recovery of the drowned; that, in fact, a monster had its abode in this fresh-water sea, and that the bodies all passed into his capacious maw. The true explanation of this mystery never has been given. The non-appearance of the bodies is due to three causes: The first is, the great purity of the water, and its consequent lack of buoyancy. Drowning is very easy in it for this reason, though I have not, while swimming in it, found any more than ordinary difficulty in sustaining myself. The second and main cause is due to the great coldness of the water. Even at this, the warmest season, the surface water is as cold as the drinker desires it to be, but it is warm there compared with its temperature at the depth of one hundred to two hundred feet. It is as cold there as the arctic cold of an iceberg. When a body sinks in the lake to the depth required, it is frozen stiff. The process, of course, preserves it, so that the gas which originates in the body from decay in other water is prevented, and distension checked. The body is thus kept in a state of greater specific gravity than the water in which it is suspended, and thereby prevented from rising to the surface. The third cause lies in the great pressure of the pure water on anything that is sunk to a great depth in it. Corks placed on deep sea nets are pressed down in a week to half their

size; and one of the oldest residents of the lake expresses the belief that, by the time a man's body has been suspended for a week at a depth of about 200 feet (it is not likely that it ever reaches the cavernous and almost fathomless bottom of the great lake), the compression of the water has reduced its size to that of a child's. Doubtless the idea of uncoffined suspension in such a "world of water" is not a pleasant one to contemplate; but to be pressed into a solid mass, and suspended in a liquid coffin of ice temperature, is quite as pleasant as interment and moldering in the ground.

THE ANATOMY OF NECROSIS.—At the meeting of the Albany County (N. Y.) Medical Society, January 22, Dr. William Hæles read a paper on *The Anatomy of Necrosis, and the Process of Repair*, illustrating the subject by the stereopticon.

The general plan of his remarks was first to treat of necrosis as it occurs in connective tissue, and to compare the processes which nature adopts in dealing with the same affection in the more compact and unyielding tissues, as the osseous and tendinous.

The subject he illustrated by a series of diagrams of the microscopical appearances of normal and inflamed bone, and a large number of photographic slides made directly from pathological specimens in the museum of the Albany Medical College, thrown upon a white wall by means of the oxy-calcium lantern. The college museum is extremely rich in the variety and number of its specimens, being one of the finest collections in the State.

The mode of separation of the sequestrum, the formation of the involucrum, the presence of the living wall of granulation tissue between the septic elements of decaying tissues and the open mouths of absorbent vessels, and the almost complete analogy existing between the various structures in accomplishing the separation of dead parts and the reproduction of the new were spoken of at length.

The microscopic and pathological anatomy of the subject was fully illustrated. The minute structure of the parts at the different stages of the affection, and the appearance of actual specimens in the various phases of necrosis were exhibited. The modifications of the vascular supply in different tissues, and their various powers of anastomosis were fully discussed.

PHILADELPHIA ACADEMY OF NATURAL SCIENCES.—The Academy now possesses more than 6,000 minerals, 700 rocks, 65,000 fossils, 70,000 species of plants, 1,000 species of zoophytes, 2,000 species of crustaceans, 500 species of myriapods and arachnidians, 25,000 species of insects, 20,000 species of shell-bearing molluscs, 2,000 species of fishes, 800 species of reptiles, 21,000 birds, with the nests of 200 and the eggs of 1,500 species, 1,000 mammals, and nearly 900 skeletons and pieces of osteology. Most of the species are presented by four or five specimens, so that, including the archæological and ethnological cabinets, space is required now for the arrangement of not less than 400,000 objects, as well as for the accomodation of a library of more than 22,500 volumes. A new building to cost half a million is now in process of erection.

A MUNIFICENT GIFT TO SCIENCE.—Some time since Professor Agassiz in an address before the Legislature of Massachusetts, called the attention of that body to the need and value of a summer school for the instruction of both teachers and students in Natural History. He also suggested that, during the coming summer, a session should be held on the island of Nantucket. These remarks attracted the attention of Mr. John Anderson, a wealthy and well-known tobacco merchant of New York, who with great munificence has donated an entire island for the purpose of the institution, supplementing his gift with a fund of $50,000. The island, which bears the name of Penikese, is of about one hundred acres in extent, and is situated in the Elizabeth group, at the entrance of Buzzard's Bay, on the southern coast of Massachusetts. It has been largely improved, and contains several buildings valued at $100,000, while the fertility of its soil is such as to render it possible to raise sufficient produce to pay all expenses of the school.

Professor Agassiz considers that the site is eminently suited for the purpose as affording ample opportunity for original investigation as well as instruction. The institution will be carried on throughout the year, in connection with the museum of Cambridge, and measures will be speedily taken to prepare the buildings for use. Arrangements have already been made to hold a school of Natural History, under the supervision of Prof. Agassiz during the summer vacation, chiefly designed for teachers who propose to introduce the study into their schools, and for students preparing to become teachers.

STATE MICROSCOPICAL SOCIETY.—A Scientific Meeting of the Society, the first of the season, was held on the evening of October 11th, 1872, President Briggs in the chair. Members present: Drs. Johnson and Curtis; Messrs. Babcock, Westcott, Thompson, Wiley, Bulloch, Adams, (W. H.) Johnson, Mrs. Fairbank, and the Secretary.

Dr. Van Heurck, of Antwerp, Belgium, Dr. R. H. Ward, of Troy, N. Y., and Mr. A. B. Tuttle, of Cleveland, O., were elected Corresponding Members of the Society.

Mr. H. H. Babcock made some remarks on the manner in which the discharge of fern spores is accomplished. According to his observations it is done by a method which differs materially from that described in the text books on botany.

A paper, by Dr. J. J. Woodward, on Nobert's Test Plate, was read by the President, after which he exhibited to the Society an optical illusion which provoked considerable discussion, and which will be hereafter again referred to.

Scientific Meeting, October 25th, 1872, at the residence of Mr. H. H. Babcock, President Briggs in the chair.

Members present: Drs. Johnson, Jackson, Curtis, Davis, (F. H.) and Adams; Messrs. Babcock, Langguth, Waters, Davis, Silversmith, Bulloch, Adams, (W. H.) Johnson, Thompson, Mrs. Fairbank, Mrs. Johnson, and the Secretary.

Mr. W. K. Steele was elected to active membership.

Mr. H. H. Babcock, presented a paper, which contained facts substantiating his previously expressed opinion regarding the existence of a current in Lake Mich-

igan, whose direction is southerly. This is proven by the existence of plants, on the beach of the southern shore of the lake, whose ordinary *habitat* is on the northern shores. Prominent among these is the *Shepherdia Canadensis*. The seeds of these plants are carried to the spot mentioned, about thirty-five miles southeast of Chicago, by the current, and there thrown on the beach.

Dr. H. A. Johnson made some remarks on the value of *Hæmatoxylin* as an imbrication for animal tissues. The use of this agent, for the purpose mentioned, was recommended by Dr. J. W. S. Arnold, of N. Y., in an article in No. 3 of the LENS. The experiments of Dr. Johnson agreed substantially in result with the statements of Dr. Arnold. The President read a paper by Prof. H. L. Smith on the *Bailey Collection of Diatomaceæ*, at present in the museum of the Boston Society of Natural History. The meeting then resolved itself into a *conversazione* for the examination of objects exhibited.

Scientific Meeting Nov. 8th, 1872, Vice-President Babcock in the chair.

Members present: Drs. Johnson, Jones, Curtis and Smith; Messrs. Biggs, Thompson, Thomas, Adams (W. H.), Langguth, Steele, Johnson, and the Secretary.

Dr. H. A. Johnson exhibited sections of a Bright's Kidney in which the arterial walls were much thickened—an inner layer of longitudinal fibers being largely developed. The patient had hypertrophy of the left ventral of the heart without valvular lesion. In presenting the specimens, Dr. Johnson stated that he had within the last few months had an opportunity of seeing the preparations of Dr. George Johnson, of Kings College Hospital, London, illustrating the same pathological condition. He remarked upon the different interpretation of their thickened walls, by Beale, George Johnson and others, and inclined to the opinion that it was a condition of true hypertrophy.

Dr. H. A. Johnson also exhibited sections of the lung of a human fœtus that was known never to have made an effort to breathe. The lung was injected through the pulmonary artery with Beale's Prussian Blue preparation, and the air cells filled with melted tallow subsequently dissolved out with ether. The object of the preparation was to show the condition of the arterial walls. In transverse sections, the arteries presented a small irregular opening with very few circular fibers and these wavy in direction, conforming to the irregular inner wall of the artery. Outside of the circular fibers there exists a thick layer of connective tissue cells and longitudinal fibers. The condition readily admits of dilatation as shown at points were the force of the injection had expanded the veins, but the artery is not expanded by artificially filling the air vesicles. In an infant that was known to have made a few efforts to breathe, a portion of the lungs was still in their fœtal state, and a portion was ærated so as to float in water. In the ærated portions the fœtal state of the arteries was not observed, but in the non-ærated portions the thick wall, and small, irregularly shaped lumen of the vessel were everywhere present.

Dr. Samuel J. Jones remarked, that the facts suggested or demonstrated by these specimens might become of value in medico-legal investigations. In artificial inspiration the lung will float, but the microscope may show that the pulmonary cir-

culation has now been established. An effort to save the life of a child by forcing air into the lungs may quite possibly produce a condition of these organs, assumed to be evidence of life, and furnishes, therefore, presumptive proof that death has taken place after birth. Dr. Johnson remarked, that he had nowhere found a description of the change in the arteries by the establishment of the pulmonary circulation. He was unable therefore to say whether or not the facts presented were new to histologists.

Mr. Babcock stated that he had received a circular from the Secretary of the State Board of Health, of Massachusetts, requesting microscopists and scientific men generally, to examine the atmosphere for indications of the then prevailing Horse Epidemic. After discussion the following committee was appointed to make examination and report at the next meeting: Drs. Johnson, Jones, Smith, Curtis, Davis, (F. H.,) and Adams.

Mr. Babcock made some remarks on the four species of *Cornus* found in this section of the country, noting particularly the coincidence of the name given by the ancients signifying horn, and the fact that the hairs on the under side of the leaf have the form of pairs of horns. Some slides were then exhibited, after which the meeting adjourned.

Scientific Meeting: Dec. 13, 1872, Vice President Babcock in the chair.

Members present: Drs. Johnson, Curtis, and Smith; Messrs. Ebert, Sargent, Adams (W. H.,) Bullock, Johnson, and the Secretary.

Dr. John Bartlett was elected to active Membership.

A verbal report was made by Dr. H. A. Johnson on the examination of the atmosphere with regard to the Horse Epidemic. The results arrived at, the Doctor said, showed nothing to which the epidemic could be traced. He thought no satisfactory result could be reached unless investigations should be periodically made for a considerable length of time, at least two years. The Society then resolved itself into an informal meeting for the examination of some new objectives, among which was a 1-16 of special merit, by Gundlach.

Scientific Meeting: January 24, 1873, Vice President Biggs in the chair.

Members present: Drs. Davis, (F. H.,) Johnson, Curtis, Adams, and Jackson; Messrs. Bullock, Johnson, and the Secretary.

A donation of six slides of pathological specimens was received from Dr. Johnson, to whom the thanks of the Society were tendered.

Dr. Adams read a letter from Prof. Sanborn, of Boston, Mass., on a new form of microscope, to be used for the examination of parts of the observer's own face. The instrument consists of an ordinary microscope tube bent twice at right angles, forming thereby a body and two arms. Inside the tube at the angles are affixed prisms or mirrors. The objective being adjusted in one arm and the eye-piece in the other, the light traversing the axis of the objective is reflected by the mirror or prism in the first angle and thrown on the mirror in the other angle, whence it passes through the eye-piece. The instrument is held in position by a clamp fixed to the middle of the body, and firmly screwed to a table or rest. The observer assumes the reclining position, and adjusting the eye to the eye-piece, brings the objective to bear on the part of the face

under examination. Sunlight is used for illumination and the objectives are, of course, low. It is the purpose of the Professor to study in this way the pathological processes involved in vesication, etc. Any one possessing a microscope can, at slight expense in procuring a tube and mirrors, avail himself of this means of study, by using his own objectives and oculars. After some discussion on the subject the meeting adjourned.

Scientific Meeting: February 14, 1873, President Briggs in the chair.

Members present: Drs. Johnson, Curtis, Adams, and Davis, (F. H.); Messrs. Thompson, Sargent, Babcock, Bullock, Johnson and the Secretary.

A paper on the *Histology of the Lobule of the Liver* was read by Dr. Curtis. The paper reviewed, at some length, the various views at present entertained on the subject, and the writer exhibited some slides which apparently showed the biliary ducts originating in channels having distinct boundaries within the lobule. The paper was to be continued at a subsequent meeting.

The President exhibited slides of a new infusiorial earth from Charlton, Mass., and stated that he had a considerable quantity of the same which he would be glad to distribute to any persons wishing the same.

After discussion, and an examination of slides, the Society adjourned.

JOSEPH ADAMS, *Secretary.*

CHICAGO ACADEMY OF SCIENCES.—The annual meeting of the Chicago Academy of Sciences was held January 14, 1873, in the library of Hon. J. Y. Scammon, Mr. Scammon in the chair. This was the eighth annual meeting since the reorganization, and the seventeenth since the commencement of the Academy.

Mr. George C. Walker then read the following report of the Board of Trustees, which was received and ordered placed upon the records;

At the meeting of the Board of Trustees of the Chicago Academy of Sciences, held this day, the undersigned, Secretary and Treasurer of the Board, was directed to submit the following report:

That early in the spring of 1872, the Board of Trustees, after a careful consideration of the whole subject, thought it best to improve the lot on Wabash avenue by building a store on the front, and rebuilding the Academy Building on the rear, with such changes as the past expecience of the Academy had made advisable.

To this end plans and estimates were prepared and made by a competent architect, and it appearing from such estimates, as nearly as could then be determined, that it would cost about $80,000 to erect the two buildings, that sum was borrowed of the Connecticut Mutual Life Insurance Company at eight per cent. interest, on a bond individually guaranteed by some of the members of the Board of Trustees, and secured by mortgage on the lot. With the money thus obtained the erection of the two buildings has been prosecuted as diligently as possible, and they are now approaching completion.

The Board cannot yet state the exact cost of the two buildings, but to complete the store ready for occupancy, and also make the rear building ready for the finishing and furnishing, will require all the cash resources of the Academy on hand, and the amounts thus borrowed.

The receipts and expenses of the Academy for the year A. D. 1872, exclusive of the cost of buildings, were as follows:

RECEIPTS.

Annual dues	$215.00
Initiation fees	20.00
Subscription notes collected	500.00
Interest	580.65
Donation	15.00
Dividend Excelsior Insurance Company	1,000.00

EXPENDITURES.

Liabilities previous to fire	$214.35
Curator and assistants	1,312.00
Purchasing and collecting specimens	292.53
Sundry general expenses	355.98
Taxes	147.92

The Board hope and expect to have the library room of the Academy Building so far completed within a few weeks as that it may be used by the curator and his assistants, and also by the Board and the Academy for their monthly meetings; and the whole building is plastered and may be made ready for its cases and fittings in a short time.

It thus becomes imperatively necessary that the Academy take some immediate steps toward the construction of permanent and fitting cases for the exhibition and preservation of its rapidly increasing store of specimens.

This will require an outlay of some thousands of dollars, to obtain which the Academy, in the judgment of the Board, should at once make a vigorous, and, until successful, continued effort.

The unpaid subscription notes amount at the present time to $5,291.25. Some of the subscribers are abundantly able to meet their obligations, and the Board urgently request them to do so, for the money is needed now more than ever before in the history of the Academy.

Thirty-one members of the Academy have neglected to pay their annual dues of five dollars for the past year, and a few are also delinquent for the year previous. Unless payment is soon made, the Academy should enforce the provision of the by-laws for such neglect.

All of which is respectfully submitted.

The donations to the library of the Academy of Sciences since the great fire have been: Bound volumes, 389; pamphlets, 925; total, 1,314: one complete set of lake charts; one complete set of coast charts; one complete set of Florida Reef charts; one engraving of J. J. Audubon. Number of foreign contributors, 55; number of home contributors, 26; total, 81.

The donations to the museum of the Academy of Sciences since the great fire have been the following: Mounted mammals, 12; mounted head of elk, 1; mammal skins, including the skin of the elephant Romeo, 16; mammal heads, 3; total, 32. Mounted birds, 770; mounted bird skins, 1,060; bird eggs, 750; bird nests, 10; total, 2,590. Skeletons of mammals, including elephant Hannibal, 10; skeletons of birds, 4; skeletons of heads, 6; skeleton of shark's jaws, 1; skeleton of python, 1; total, 22. Mineral specimens, 126; fossil shells, 2.000; recent shells, 4,000; botanical specimens, 500; mounted fish, 28; mounted tur-

tle, 1; insects, about 1,000; 2 five-gallon cans and 20 jars and bottles of specimens in embryology, reptiles, crustaceæ, mollusca, radiates and insects; 2 barrels of coral from Florida.

There is also quite a large collection now packed in boxes, which it is not proper to open until the cases are prepared to receive the specimens.

The Academy then proceeded to the election of officers for the ensuing year, the following being duly balloted for and elected: President, J. W. Foster, LL. D.; Vice Presidents, E. W. Blatchford, Dr. H. A. Johnson; Recorder, Dr. Norman Bridge; Librarian, Dr. J. W. Viele; Committee on Membership, Dr. E. Andrews, E. W. Blatchford, Dr. John H. Rauch; General Committee, Dr. J. W. Foster, Dr. E. Andrews, E. W. Blatchford, H. A. Johnson, G. C. Walker, S. A. Briggs, E. H. Sargent, Wm. Bross, Dr. A. E. Small, H. H. Babcock.

Among the objects of interest exhibited to the meeting were some plates accompanying the work of Captain Charles M. Scammon, United States Revenue Marine, on the natural history of cetaceans and other marine mammals of the western coast of North America. The plates have been pronounced by Prof. Baird, Prof. Agassiz, the State Geologist of California, and other sompetent authorities, to be the finest ever published. There was also exhibited a case of reptiles, co'lcctcd by Major Robert Kennicott, in Michigan, and which has been in the possession of Dr. Davis. Specimens of artesian well water from the new well in Scammon Court were also seen and tasted, and passed upon.

Messrs. Albert E. Ebert, Norman Bridge, and George F. Rumsey, the special committee appointed to solicit funds, have issued the following circular appeal, which ought to receive a generous and early response from every member and friend of the Academy:

CHICAGO, February 1st, 1873.

DEAR SIR:—At the annual meeting of the Academy of Sciences, held Jan. 14th, 1873, it was resolved that the help of all those interested in Science in this community, be solicited to re-establish the Academy in its former position of usefulness.

The Academy buildings have been replaced in the same location, and superior in all respects to those destroyed by the fire. This expenditure having exhausted all the available resources of the Academy, it now becomes necessary to appeal to those interested, to supply the means required to make the interior ready for the reception of the large collections of books and specimens pertaining to the Natural Sciences, now being received from kindred Societies and individuals in all parts of the world.

For this purpose there will be required, cases, fixtures, containers, etc., involving an outlay of at least $20,000.00.

The Chicago Academy of Sciences has heretofore held a prominent position among the scientific institutions of the country, and we confidently appeal to the citizens of Chicago to aid in maintaining its reputation, and furthering the progress of scientific research, thereby keeping pace with the extraordinary recuperation of this city.

You are respectfully asked to make such a donation as you deem proper, for this purpose, by filling the accompanying blank and returning it to the Treasurer of the Academy, GEO. C. WALKER, ESQ., Chamber of Commerce Building, as soon as convenient.

The committee hope, by this appeal, and by personal solicitation, to raise the amount necessary, during the present season.

SAN FRANCISCO.—The Microscopical Society of San Francisco, Cal., held its annual meeting on the evening of February 11, at the Society's rooms on Clay street. Dr. A. Kellogg presented for examination a section showing the reticulation of the cuticle of the bulb-covering of the onion.

General Hewston presented a sample of infusorial earth, containing some rare forms of Diatoms.

The new seal of the Society was presented and adopted.

The following officers were unanimously re-elected for the ensuing year: Henry G. Hanks, President; Arthur B. Stout, M. D., Vice President; C. Mason Kinne, Recording Secretary; Henry C. Hyde, Corresponding Secretary; D. P. Belknap, Treasurer.

Owing to the numerous applications for membership, a resolution was adopted increasing the limit of membership to fifty.

The meeting then adjourned.

RUSH MEDICAL COLLEGE, CHICAGO.—We are glad to note in the announcement for the spring course that this institution continues the *Concour* system, which has heretofore been so successfully pursued.

[Advertisement.]

MR. WENHAM AND THE TOLLES' TENTH.—In the discussion of angular aperture, Mr. Wenham writes to the *Monthly Microscopical Journal* (March, 1873), respecting the $\frac{1}{10}^{th}$ objective of Mr. Tolles' make, of which he professes to have measured the angular aperture, that when the lenses were brought as close as the arrangement of adjustment would allow, "then the aberrations were such that it would not define objects under any thickness of cover. The maximum angular aperture of the objective is not at that point. As the owner and constant user of the instrument for three years, I believe that I know its character as well as Mr. Wenham. The idea conveyed in Mr. W.'s letter is, that the objective will not define well at the maximum angle, but only at the adjustment he fixed on. This position I claim is utterly untenable. It may be believed in England, but I do not intend to send the objective there again for examination. If there are any microscopists—English or American—that have faith in Mr. Wenham's results, I now invite them to call on me and look through that instrument themselves. If there is any $\frac{1}{10}$ objective that will work through a thick cover and give sharper definition, I have yet to see or hear of it.

CHARLES STODDER

Boston, March 17th, 1873.

THE LENS;

A Quarterly Journal of Microscopy and the Allied Natural Sciences,

WITH THE

Transactions of the State Microscopical Society of Illinois.

VOL. II.—CHICAGO, AUGUST, 1873.—No. 3.

*THE SILICEOUS SHELLED BACILLAREÆ OR DIATOMACEÆ.**

I.—PRELIMINARY PART.

1.—*Historical Introduction.*

ALREADY for four thousand years had the mind of man searched the wonder works of creation, yet a vast field remained unexplored, closely connected with the numerous forms of that endless Nature which the unaided eye had recognized, and the higher probing mind had arranged; then, in the commencement of the 17th century, a compound microscope was invented by Zacharias Janson and his son, in Middleburg, and with that men ventured upon the unknown, and till then, invisible field of smallest organisms, the discovery of which opened an entirely new world in miniature.

The Diatoms, or Bacillariæ, whose natural history is given in this work belong to these minute microscopical objects.

Although it is uncertain what particular forms of the diatom group, the first observers found, and endeavored to represent, by description and picture, yet it may be taken for granted, with great certainty, that they must have met with isolated specimens, since they are so numerous and widely distributed.

*The introduction of Kutzing's Bacillariæ presents so many points of interest for the student, and is so valuable as an historical summary, that I propose in the intervals between the appearance of the different parts of my own Synopsis, to give a somewhat free, though accurate, translation of it.

For the first discovery of forms belonging here, which are, in some measure, given with certainty, we have to thank O. F. Müller, who described and figured a *Gomphonema* in 1773 as *Vorticella pyraria*, and in 1783 a *Fragilaria* as *Conferva pectinalis*, also a *Melosira* as *Conferva armillaris*. A much greater sensation was made by the discovery of the so called staff animalcules (*Vibrio paxillifer*) by Müller, and which the discoverer, at first, did not know where to classify, but later embodied it in the genus *Vibrio*, in his large work on Infusoriæ.

Gmelin, in the 13th edition of Linne's "*Systema Naturæ*," correcting the mistake, founded a special genus upon this form, to which he gave the name *Bacillariæ*, and from this, the whole group received from Zoologists the name of *Bacillariæ*, or staff-animalcules.

The great similarity of several *Bacillarian* forms to Conferva, soon caused the Algologists to pay more attention to them; already, indeed, O. F. Müller, himself the first of living infusoria investigators, had declared his *Conferva pectinalis*, and *C. armillaris*, to be Algæ.

The lower Algæ had, at the end of the last century, very zealous friends in Germany, in Mertens, Trentpohl, Roth, Weber, and Mohr; in England in Dillwyn, and in France in Girod-Chantrans and Draparnand; and several forms, now distributed among the genera *Fragilaria*, *Melosira*, *Tabellaria*, *Diatoma*, and *Schizonema*, were described by these naturalists as confervæ.

The knowledge of these forms at the beginning of this century, was increased almost entirely by the Algologists, and among the illustrations furnished in the *Flora Danica*, the *English Botany*, and the large copper-p'ate work of Dillwyn, were several Bacillariæ mentioned as confervæ; but while the figures of Dillwyn, and the *Flora Danica*, left room for many improvements as to the representation of the exacter microscopic proportions, those of the *English Botany* were better; and were especially good on the figures of *Conferva stipitata* (Tab. 2488=Achnanthes longipes), *Conferva obliquata* (Tab. 1869=Isthmia enervis), *Conferva Biddulphiana* (Tab. 1762 =Biddulphia pulchella).

Although DeCandolle, so far as is known, made no special study of these organisms, yet he was the first who separated the form previously known as *Conferva flocculosa*, as a special genus which he

called *Diatoma*. Agardh followed DeCandolle in this, inasmuch as he incorporated this genus into his "*Synopsis Algarum*, 1817," but he combined with it other species, (*D. Swartzii*, *D. pectinalis*, and *D. fasciculatum*), which are now distributed among as many different genera.

For the most important investigations, however, which were made of the Bacillariæ the same year, we have to thank Nitzsch, and rightly does Ehrenberg call them "classic." He furnished in his little work, long since out of print, "*Contributions to the Knowledge of Infusoriæ, or a Natural History of the Zerkariæ, and the Bacillariæ, with six colored copper-plates*, Halle, 1817," the first really good pictorial representations, and recognized first the prismatic shape of these forms (which he mentions as a principal character of the group). He carefully observed the propogation of the staff (Stäbchen) through length division, from which he explained, quite correctly, the separation of certain forms in the peculiar zigzag like chains, as also the production of ribbon-like forms, from an imperfect separation. He showed the unchangeable character of the external parts, after death, and also distributed several new species, associating however, very different forms, from his personal dislike to minute distinctions. All these forms he placed in two main groups, viz.: vegetable and animal, the former containing those which appeared to him immovable; later observations however, have shown that most of his vegetable species, possess also voluntary motion. Two years later, 1819, appeared Lyngbye's "*Tentamen Hydrophytologiæ danicæ*," a work which, for that time, was of the greatest importance. In this, more Bacillarian forms were described, and figured, than had been done hitherto in any other work. Twenty-five different forms were distributed among the genera *Diatoma*, *Fragilaria*, (a new arrangement of Lyngbye) and *Echinella*. The name of this last genus had been previously given by Acharius (in Weber's *Aid to Natural History*, 2d Vol., p. 240) and incorporated for several years in the "*Systematic Handbooks*," and had even been given out by me in my "*Decades of Fresh Water Algæ*," to a form which, in the following year, 1835, was recognized as insect eggs; but the genus of Lyngbye of this name did not contain the true form of Acharius, from which the name had been transferred to quite a different plant form; also the remaining *Echinelleæ* of Lyngbye were foreign to, and only a few

exhibited the prickly aspect of, the form of Acharius. Soon after, 1820, Link (In the *Horæ Physical berol.*) described two genera, *Lysigorium* (=Melosira) and *Hydrolinum* (=Schizonema.)

Bory de St. Vincent wrote for the "*Dictionaire Classique d'Historie nat.*" the article "*Arthrodiées*" which appeared in 1822, and besides *Oscillaria*, *Conferva* and *Zygnema* treated also of some *Bacillariæ*. In this article, the *Echinella stipitata* was described, and figured, as *Achnanthes stipitata*; he placed however in this genus other forms not belonging to it. The genus *Fragilaria* of Lyngbye he described as *Nematoplata*; the genus *Diatoma* was enriched by a new species, and a fourth genus constituted under the name *Stylaria*, which contained chiefly Gomphonema forms. In the article "*Bacillariées*" by the same author the genus *Navicula* was constituted, and in the article "*Confervées*," which appeared in 1823, he described the genus *Gallionella*.

But while Bory de St. Vincent founded his genera chiefly on the researches of other students; and the few investigations of his own, conveyed mainly the impression of superficial work; the labours of C. A. Agardh in the same group appear to much better advantage. He in his *Systema Algarum*, 1824, mentions the Bacillariæ as a special order of the Algæ under the name "*Diatomeæ*," and better and in a more thorough manner than his predecessors, he arranged them in the genera: 1, *Achnanthes*; 2, *Frustulia*; 3, *Meridion*; 4, *Diatoma*; 5, *Fragilaria*; 6, *Melosira* (= Gallionella, Bory); 7, *Desmidium* (which we however exclude); 8, *Schizonema*; and 9, *Gomphonema*.

In the year 1827, C. A. Agardh described in the Regensburg *Botanical Journal*, Nos. 40 and 41, several diatoms newly discovered by him in the Adriatic sea, and at Carlsbad, on which occasion, he mentions for the first time, the genera *Micromega*, *Licmophora* and *Homœocladia*. The same Algologist wrote most particularly on this family in four condensed theses which appeared with a common title, "*Conspectus Criticus Diatomacearum.*" In the first and second (1830) he described a great number of forms, partly already known, partly new, under the genera: 1. *Cymbella*; 2, *Schizonema*; 3, *Micromega*; 4, *Berkeleya* (which was first brought forward by Greville in 1827); 5, *Homœocladia*; 6, *Gloeodictyon*; 7, *Hydrurus* (which genus is excluded by us here); and 8, *Gloeonema* (under which the author united very different organisms). In the follow-

ing, third part, (1831) he gave the genera: 9, *Gomphonema;* 10, *Styllaria* (= Podosphenia, Ehr.); 11, *Meridion;* 12, *Lichmophora;* and 13, *Frustulia;* in the last part, (1832). the genera: 14, *Isthmia;* 15, *Odontella;* 16, *Desmidium;* 17, *Achnanthes;* 18, *Striatella;* 19, *Fragilaria;* 20, *Grammonema* (belonging to the Desmidieæ); and 21, *Melosira.* In the whole, the author described (excluding the forms without siliceous shells, and not belonging here) about 116 species. It should be remarked however here, that before the appearance of this last work by Agardh, some very good investigations were published by Leiblein in the Regensburg *Botanical Journal,* concerning several diatoms, which Agardh incorporated into his conspectus. While Greville had already described (1827) in the 5th Vol. of his "*Scottish Cryptogamic Flora*" the genera *Exiiaria, Monema,* and *Berkeleya.* Turpin founded the genus *Surirella* in 1828, and Gray in 1830 the genus *Biddulphia,* from *Conferva Biddulphiana,* and *C. Obliquata* of the *English Botany.* Thus till the year 1832, stood the systematic labours on these microscopic organisms; most of the writers mentioned, considered them partly as animals (the moving forms,) and partly as plants (the fixed forms). Only Agardh, Lyngbye and Leiblein declared more decidedly for their vegetable character; but besides Schrank, there was not one who decidedly advocated their animal nature; of their inner constitution, and of their life-relations, nothing was known, beyond the thorough communications by Nitzsch, already mentioned, and some superficial observations by Gaillon, that might have brought the question, as to their nature, nearer its solution. In the same year (1832), appeared the second "*Contribution to the Knowledge of the Minutest Organisms,*," by C. G. Ehrenberg. In this the Diatomaceæ were considered as decided as animal forms; the 43 species observed by the author himself, were distributed among the genera: 1, *Navicula* (= Frustulia, Ag.); 2, *Bacillaria* (=Diatoma, Ag.); 3, *Fragilaria;* 4, *Exilaria* (= Meridion, Ag.); 5, *Synedra* (= Exilaria, Grev. = Diatoma and Frustulia, Ag.); 6, *Gomphonema;* 7, *Cocconema;* 8, *Echinella* (= Licmophora, Ag.); they were all incorporated with the infusoriæ under the family of "staff animals," "(*Stäbthierchen*)" (including the *Desmidieæ*) in the class of "stomach animals" (*Magenthiere*).

But at that time, stomachs were as little recognized by the author, as mouth, entrails or rectum; but a bivalve shell, (*panzer*) and a

changeable foot (as in the Gastropods) was mentioned, and said to stretch out of the longitudinal cleft of both valves. Another communication from the same author, followed in 1834, being his third "Contribution," in which were described 16 newly observed forms. The descriptions communicated in these observations are of the greatest importance and are given with a care hitherto unknown in this field. The author had this advantage over most of his predecessors, that in his investigations he could make use of the best microscopes. Within *Navicula Amphisbæna*, he considered the colored substance as an ovary, and took the lighter cysts appearing therein as polygastric stomach sacs; at the same time he pointed out that a bivalved, grooved shell, as Turpin has shown it in *Surirella Striatula*, "was in plants, something without analogy, but allying itself easily to animal forms," and yet, just this circumstance had decided Turpin, who knew right well that there were also striped and manifold marked plant cells, to consider the form mentioned among the "vegetabilia." Lastly, he called attention to an essential characteristic of the Bacillariæ which had already been correctly understood by Nitzsch, but falsely represented by Agardh and other Algologists, namely: Agardh supposed that in a diatom the little staves (*Stäbchen*) united themselves lengthwise by twos in the beginning, then separated, and cohered only at the ends, but Nitzsch had already shown that the forms united at the edges were produced by imperfect self-division, an opinion which was pronounced also by me in 1833, and which Ehrenberg confirms.

In the year 1838, appeared the great work by Ehrenberg, "*Die Infusionsthierchen als Vollkommene Organismen.*"—"The Infusoria as perfect organisms." The author had, already, previously published several observations on the diatoms, which we here consider with the others. He was the first who showed openings in the hard valve, (*Schale*) (the central one was interpreted in many frustules as mouth openings.) Under *Navicula*, the snail foot-like organ of motion was again mentioned, and which in most cases, is said to protrude from the valve. The larger, brighter cysts in the colored ovarian mass, were decided to be "stomach cells" because the author, after many years of experimenting, succeeded at last in observing the reception of colour in them. Lastly, he mentioned also, egg-like colourless granules, which he thinks are to be taken for sexual organs. The ribbon-formed and other combinations of

individuals into a whole, he compared with monads, or Polypi stems.*

Farther on we shall notice the merits which the author has gained by his diligent investigations of the fossil forms, and the influence which these minute organisms still exercise upon our earth; here, only a few words must be added on the systematic arrangement of the group as contained in the great work on the Infusoriæ. Since the first attempts to bring the diatoms into several genera, the outward form of the shell-covered body, the manner in which the single individuals united, and the presence or absence of a stipes whereby they are attached, have been principally taken as the foundation in the classification, and Ehrenberg introduced also, the presence or absence of the shell-openings, for the distinction of genera, but the main groups were arranged according to the presence or absence of a stipes; a mistake which caused the author to mention Lyngbye's *Diatoma arcuatum*, not only as two different species, but also under two different genera, viz.: as *Tessella catena*, and *Striatella arcuata*. His 154 species, contained in the work already mentioned, and mostly accompanied with very carefully drawn figures, form, with him, the group "*Naviculacea*," and are distributed among the following genera: 1, *Pyxidicula*, (= Cyclotella, Kg.); 2, *Gallionella;* 3, *Actinocyclus*, new; 4, *Navicula;* 5, *Eunotia*, new; 6, *Cocconeis*, new; 7, *Bacillaria;* 8, *Tessella*, new; 9, *Fragilaria;* 10, *Meridion;* 11, *Isthmia;* 12, *Synedra;* 13, *Podosphenia* (= Stylaria, Ag.); 14, *Gomphonema;* 15, *Echinella*, (= Licmophora, Ag.); 16, *Cocconema;* 17, *Achnanthes;* 18, *Striatella;* 19, *Frustulia;* 20, *Syncyclia*, new; 21, *Naunema* (= Schizonema); 22, *Gloeonema*, (= Encyonema, Kg.); 23, *Schizonema;* 24, *Micromega*.

Of Ehrenberg's works, published subsequently, giving an account of his continued investigations of the siliceous shelled diatoms, the following are particularly of importance: 1st, "*The formation of the European, Libyan and Arabic Chalk Rocks, and the Chalk Marl from Microscopic Organisms*," (contained in the "*Proceedings of the Berlin Academy of Sciences*," 1839). In this communication the new genera *Coscinodiscus*, and *Dictyocha*, with several species, and some new fossil species of the genera, *Actinocyclus*, *Cocconema*, *Denticella*, *Fragilaria* and *Navicula*, were described. 2d, "*On*

*One need scarcely to remark that all these observations of Ehrenberg are wholly fanciful. —*Translator.*

Numerous, still living Species of Animals of the Chalk Formation," (also in the *Proceedings of the Berlin Academy*, 1840). In this pamphlet, Ehrenberg showed that many diatoms hitherto found by him only in the fossil condition, were living in sea water, especially in the slime of the coast. Most of them he had gathered near Cuxhaven. Of great importance, however, was the observations of the organs of motion in the *Navicula gemma*, which shall be mentioned presently. At the same time the genera *Amphitetras*, *Ceratoneis*, *Grammatophora*, *Lithodesmium*, *Podosira*, *Triceratium*, *Tripodiscus* and *Zygoceros*, were newly established, and a considerable number of new species described, which are partly represented in the appended copper plates. 3d, "*Brief account of 274 newly observed Species of Infusoria, since the completion of the plates of the larger work on the Infusoria,*" in the reports of the Berlin Academy of Science, 1840, in which about 100 new species of diatoms were described, and the genera *Amphipentas*, *Campylodiscus*, *Discoplea* and *Himantidium* were established. 4th, "*Extent and influence of Microscopic life in North and South America,*" 1840; without doubt, the richest of the last named works, and at the same time furnished with many figures, on four copper plates.

Prof. Bailey of West Point, had already, in 1838, given the outlines of American Bacillariæ in *Silliman's Journal of Science and Arts*, Vol. 41, No. 2, and Vol. 42, No. 1, and had also, especially reported on the fossil forms of North America. Abundant material was sent to Ehrenberg from this continent, from thirteen different localities, and at the same time, he received contributions from South America, through his brother, Carl Ehrenberg, and besides this, he knew how to obtain samples of earth from different other points of that continent, which were brought to Europe in the transportation of lumber, so that he obtained a view of the forms from 44 different localities in America, from the Falkland Islands to the Kotzebue Sound. Lastly, some forms from Spitzbergen and Iceland are given. The number of species described as new, is pretty large, several American species however, mentioned as new ones, could have been reduced to European species, and also from these communications it is proved that in the remotest places, the same forms of Bacillariæ are usually repeated, and the remarkable differences appear only singly, and seldom.

The genera *Actinoptychus*, *Amphiporora*, *Climacosphenia*, *Goniothecium*, *Mesocenia*, *Rhizosolenia*, *Sphenosira*, *and Tespsinoë*, are

mentioned as new, also the separation (not happily) of *Pinnularia* from *Navicula*, and besides, 227 new species are described, most of them figured; and also incorporated into my own plates. I shall, however, have frequent opportunity to refer to these, and all other labours of Ehrenberg, wherefore for the present I finish my notice of the great industry of this man, who has at command in his lucky position, all possible means for the prosecution of his scientific investigations.

The same year in which Ehrenberg's large work on Infusoriæ appeared, A. de Brebisson published his "*Considerations Sur les Diatomées.*" Brebisson had diligently studied the Algæ of his neighborhood, (*Falaise,*) and had spent much time in hunting up the little diatoms. Of many of the new species whose names only he mentions in that brochure, he has given specimens to his friends in Germany: by the use of the specimens I was enabled to get the necessary information regarding them. Upon the whole, his classification is very near that I had made in my *Synopsis Diatomearum*, 1833, only some subdivisions of my genera he made into independent genera, as for instance, *Cymbophora*, (= Coccenema, Ehr.) *Cyclotella*, (= Pyxidicula, Ehr.) and besides that, *Epithemia* (which corresponds with the genus Eunotia, Ehr.) and *Surirella*.

Besides these, also Greville (in Hooker's *British Flora*, II) and Harvey in the "*Manual of British Algæ*," have lately become co-workers among the Diatomaceæ, but in a manner which still reminds of the times of Lyngbye and Agardh, so that their labours are almost entirely useless for our purpose, because they lack the necessary strictness. The latest discoveries remained quite unknown to these men, at least they had no influence on their labours.

Ralfs has furnished the most recent work on British diatoms, in single monograph, which is printed, and accompanied with figures, in the 12th Vol. of "*Annals and Magazine of Natural History.*"

Ralfs excels his predecessors in his knowledge, and better representations of separate forms; he has also used the publications of others better than his countrymen just named, but the figures on most of his plates, (only with the exception of Pl. 8, which contains beautiful and successful representations of the genera *Amphitetras*, *Biddulphia*, and *Isthmia*) are pretty crude; it seems, however, as if this was more the fault of the engraver than of the author.

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THE CELL.

IV.—THE PROTOPLASM OR FORMED MATERIAL.

IN passing from the consideration of the nucleus or germinal matter to the formed material, we cross an abrupt and sharply drawn boundary; it is that boundary which separates matter capable of growth and development merely, from matter which is capable of functional activity merely. In other words, we pass from the study of matter which can only increase and multiply itself, to the study of matter which can only exhaust itself, or wear itself out, for the benefit of the body as a whole. Since, therefore, the formed material must be fitted for the performance of various functions, we can readily understand that it will be likely to present a more varied structure than we encountered in germinal matter.

In fact it is the structural variations in the formed material which gives us the different types of cell growth. If cells were never developed beyond the nucleus or germinal stage, we should have no variety at all; every cell would be precisely like every other cell in structure, and, for aught we know, in potentiality.

Somehow, we know not how, cells or living masses, which look precisely alike in their infancy, acquire, during the progress of their development, certain distinctive structural differences, and at the same time they assume certain specific duties. Hence we encounter various types of cell growth; but these types depend upon variations in the form and consistency of the formed material—not upon variations in the germinal matter.

The term "formed material" was first brought into use by Beale, and was by him made to comprehend all that portion of the cell lying outside the nucleus. It has also been called "cell contents" by various authors, "periplast" by Huxley, "sarcode" by Dujardin, and "protoplasm" by Max Schultze, Rindfleisch and others. Of late years writers are much in the habit of calling it "protoplasm," not because this term presents any special advantages over several others, but rather because, since Huxley's famous lecture on "protoplasm," this term has come to be greatly "in fashion," so to speak. If to-morrow, Huxley or some other "star" of the first magnitude should flash a new term upon us, we should immediately drop the old plaything and rush unthinkingly for the new.[1]

(1) The term protoplasm has also occasionally been made to include the nucleus; in fact it has been and still is used very loosely. In the majority of instances, however, it is used in connection with formed material only.

The study of the formed material involves the attempt to answer these three questions: *First*, What is its structure? *Secondly*, What are its functions? *Thirdly*, Is it living or dead? The first two queries present themselves as a matter of course; the third is rendered to a certain extent inevitable, by Beale's broad and somewhat dogmatic assertion that the formed material is always non-living matter. I shall consider these questions in their order:

First. What is the structure of the "formed material"?

In the first place we encounter the fact that it is not, like the germinal matter, constructed after any rigid or inflexible rule. Its structure is as variable as are the duties it is required to perform. Some of these duties are essentially active or vital; others are merely passive or mechanical; some require a solid or semi-solid formed material; others require a fluid formed material; hence the necessities of the case demand that the formed material shall exist under a multiplicity of types, instead of one general and unchangeable type.

We find fluid protoplasm in all secretions; notably in saliva, mucus, bile and pancreatic juice.(2) It is the direct and completed product of the gland-cells, and is by them formed for a specific purpose. It has passed through the period of germinal matter—(the era of its childhood), and has now reached its perfect period of development, or its adult period. The change, therefore, from semi-solid germinal matter, to fluid formed material, is not necessarily, or indeed generally, a retrograde step, but rather a step in the opposite direction. It is the completion of the work of development. Another variety of "formed material" is the horny substance which forms the greater part of the cells composing the epidermis and the various epithelia; this substance gives no evidence whatever of "structure," in the ordinary acceptation of that term; it seems to be the result of the simple solidification or dessication of the germinal matter, a minute portion of which can still be seen in the centre of the cell.

Advancing another step, we come to the formed material of cartilage, hyaline or faintly granular, presenting no evidences of structure, fitted for duties which are merely mechanical or menial, and evidently the product of a sort of physical mutation of the germinal matter from which it was derived.

(2) The *secretions* must not here be confounded with the *excretions*: they are totally dissimilar, both as regards origin and destination.

A necessity arises for a solid material, which shall act as a framework for the body; hence in the bones we meet with peculiarly formed cells, their formed material being composed mainly of calcareous salts in fine particles, combined with, or associated with a small proportion of animal matter. It is not essential for us to know whether these calcareous particles are the result of the conversion of the original germinal matter, or whether they are the product of simple infiltration or precipitation; in either case the effect is the same; the germinal matter undergoes gradual diminution, while the formed material is gradually increased;—and as a consequence of this, that which *was* cartilage is replaced by that which *is* bone.

Another modification of the formed material we find in that which, in its perfect form, becomes simple fibre—as in white and yellow fibrous tissue; fibres being constructed by the welding together of individual cells which have first elongated, then become spindle-shaped, and lastly drawn out into exceedingly minute *fibrils*, which by their aggregation form fibres. It is noticeable that in the process of development, these two kinds of formed material acquire physical properties which are totally unlike; the *yellow* fibre being very elastic while the *white* fibre is invariably rigidly inelastic. Up to this point we have only encountered formed material capable of manifesting physical characteristics; that is we have met with no form of tissue which *needs* to be endowed with vitality to enable it to perform its duties in the economy. None of the tissues which we have glanced at, require anything of vital force, except in the interest of their own nutrition and repair, and this is invariably attended to by the small proportion of germinal matter which they each still contain, and which *remains* germinal matter for the special purpose of maintaining nutrition and repairing waste.

In passing to the higher forms of protoplasm, we first meet with a tissue possessed of the power of voluntary contraction—namely the muscular tissue. Its proper formed material consists of the so-called "musculine"; in this substance, therefore, we are to look for the power of contractility. The formed material of muscles is thrown into minute fibres; these are divisible into yet smaller fibres, which are called fibrils; these again are separable into exceedingly minute disks or segments, the so-called "ultimate sarcous particles" or "sarcous elements" of Bowman. To the microscopist then, these minute particles are ultimate; they are the

elementary structural form of the contractile material of voluntary muscle. In the involuntary or unstriped muscle, we find that the spindle-shaped cell is the ultimate structural element, and that the muscular tubes and planes are produced by the weaving together of these elongated contractile cells.

The highest development of formed material is that which is encountered in the nervous system. Essentially, it consists of the cellular portion, which is the active and potential part of the nervous centres; and the fibrous or tubular portion, which pervades the entire body, and conveys impressions both to and from the nerve centres. In the cells, we find formed material of a grayish or ash color, in large quantity, presenting an indistinctly granular appearance under high powers, surrounding a minute island of germinal matter, from which it was developed and by which it is maintained. In the fibres, the "axis cylinder" is the peculiar formed material, and it is merely drawn out into long, minute cords, instead of being massed around the nucleus.

Under these diversified types do we find the products of cell growth, when they arrive at their last period of development. It must be borne in mind that these various forms of protoplasm are *all* the product of the growth of the germinal matter, and developmental power resident in it. But the power of growth alone is not sufficient; for, if the germinal matter of any two or of any six tissues be compared, with the utmost care, and with the highest magnifying powers, no essential difference can be perceived; hence there must be a peculiar power of differential development, in addition to the mere power of assimilation and growth.

Secondly. What is the function of the formed material?

This question has been partly answered already. Certainly the formed material has nothing to do with growth, development, assimilation, nutrition or repair; else it would not be "formed" but rather "forming" material. Two necessities are constantly present; two demands constantly before us; the one for a something which shall develop, nourish and repair—or in other words, replace materials which are worn out and wasted; and this demand is fully and adequately met in the germinal matter; the other for a something which shall carry out the designs of development, or carry on the various functions, and execute the various duties necessary for the good of the economy; and this demand is fully and adequately

met in the formed material. The formed material, then, is never concerned in growth and development, but always in function; it never possesses the power of increasing itself, or even of maintaining its integrity; but it is always engaged in wearing itself out by active labor or passive service for the general good. Hence we find it under a multiplicity of forms, each individual form being adapted to a special end.

Of course a large proportion of every full-grown living being is made up of formed material, and the comparatively small proportion which is composed of germinal matter, is utterly useless for any purpose except that of assimilating pabulum for the replacing of worn out formed material; it is powerless for any other purpose.

So long as the body of any animal is composed exclusively of germinal matter, (as in the earliest period of embryonic life) it possesses no power beyond that of assimilation and growth, and, possibly, that of amœboid movement; it has not yet acquired any faculty which may properly be classed as "functional," nor will it, until a specifically endowed formed material shall have been created.

The formed material, then, must be regarded as the true repository of functional *power* when the exercise of function demands the exercise of *power*, as in contraction, innervation and intellection; and it must also be looked upon as the passive servant, when its functional duties require it to passively occupy the place of a menial merely, as is indeed the case with the formed material of epidermis, the epithelia, and of cartilage and bone.

Thirdly.—Is the Formed Material living or dead?

According to Prof. Lionel S. Beale, it is always dead, it has passed the boundary which separates dead from living matter. I quote his own words in proof of this: "To avoid entering into a long and tedious discussion as to the meaning which should be assigned to the words in general use, I have been led to use new terms when speaking of the essentially different parts of the cell or tissue. I apply the term *germinal matter* only to that which *lives, changes, converts, germinates, etc. Formed Material,* on the other hand, never possesses any of these properties. *It has lived,* but is now lifeless; it may *be* changed, but it cannot change itself."(3) Again he says: "The terms *living* and *dead* have for me a meaning somewhat different from that commonly accepted. If my arguments are sound, the greater part of the

(3) How to work with the Microscope, 4th Ed., page 318.

body of an adult man or animal, at any moment, consists of matter to all intents and purposes as dead as it would be if the individual itself were deprived of life. The formed material of the living cell is dead. The only part of the living cell and the living organism which is alive, is the germinal matter. Nothing can be regarded as alive or living but germinal matter in which vital changes alone take place."[4]

These are strong statements, and if they are accepted as true, we must regard ourselves as constantly more dead than alive. For my own part I neither wholly accept or wholly reject them. Germinal matter is, without doubt, *always* living matter. Formed material, on the other hand, is sometimes living, sometimes dead; this is a question which is determined not by the mere presence or absence of the power to "change, convert, germinate, etc.," (as Dr. Beale believes), but rather by the quality of the duties which it is created to execute. If it is intended for merely passive or mechanical functions, as in the case of the formed material of epidermis, cartilage, and white and yellow fibrous tissue, it is not living but dead. It has no necessity whatever for life; indeed the formed material of all the lower tissues is far more servicable, if it be deprived of vitality and therefore of sensibility, than it would be if living and therefore acutely sensitive. Think of the exquisite torture of a vital and sensitive epidermis, or cartilage, or ligament, or bone. Dead, and non-sensitive, these tissues (or these varieties of formed material) are of the highest utility in the economy; living and sensitive, they would simply make existence unendurable. These lower and passive forms of formed material simply need that amount and that kind of vital force which enables them to superintend their own nutrition, and this is supplied to them, as I have already pointed out, by the minute masses of germinal matter which always remains unchanged or unconverted, without doubt, for this single and very important purpose. But if we glance at the "higher animal tissues"[5] (particularly muscular and nervous tissues) we shall find conditions essentially different, demands vastly higher, and functional requirements which non-living tissues cannot fulfill. The formed material of muscular tissues must and does possess the inherent power of contractility; out of this we get the power of locomotion and other faculties,

(4) Op. Cit. p. 329. The italics are Dr. Beale's.
(5) Virchow: Cellular Pathology, Chance's translation, page 77.

without which we should be worse than useless. Two things seem to me absolutely certain: first that I do not walk, or do any other act involving muscular contraction by means of dead muscles; secondly that my power of locomotion (muscular contraction) does not depend upon the germinal matter which I find in muscular tissue, since its quantity is too minute to admit of its exercising any contractile power. I am therefore forced to the conclusion, in spite of Dr. Beale, that muscular tissue is living tissue; that while it no longer possesses, and no longer needs, a developmental power or life, it *does* possess, and *does* need, a functional power or life; that when it passed the boundary which separates germinal matter, (matter endowed solely with developmental or germinating power), from formed material (matter endowed solely with functional—in this case contractile—power), it carried over with it just so much of vital force as should last it through the period of its functional life; and that, just so long as it is capable of executing its allotted duties as a constituent element of the body, it is and must be undeniably living.

If the formed material of muscular tissue is dead—or if Prof. Beale chooses, "non living," while it is yet capable of contraction, what change comes over it when it passes into that truly lifeless and helpless condition which we have been accustomed to call death? Again, how are we to tell when and where life ends and death begins? Indeed, for aught we know, half the people we meet walking about the streets, are dead already, and ought to have been buried long ago, if Dr. Beale's views are correct. Prof. Beale attributes contractility to a "disturbance (electrical or otherwise) in the neighborhood of a contractile tissue." The answer to this is, that no "disturbance," whether "electrical or otherwise," can induce contraction in a muscle which is truly dead.

The same ideas hold good as regards the nervous system. Nervous influence is generated in and by the formed material of the cells of nervous tissue. This is a truly vital act; we know that death has occurred because the nervous system fails to respond to the usual vital stimuli. Intellection is the highest attribute of vitality; but the power of intellection has its seat in the formed material of the cells of the gray matter of the cerebrum; can we make ourselves believe that we do our thinking with dead brains? Manifestly not. But the adoption of Prof. Beale's sweeping dogma concerning the status of the formed material, leads us to precisely this conclusion, in spite of ourselves.

While growth and the power of development is undoubted evidence of life, the absence of the power of growth and developement is not absolute proof of death; indeed it *may* be simply the proof of a more mature life. The attempt therefore to make the presence or absence of developmental power the test of the presence or absence of life, cannot be admitted as valid or reliable. If growth were the only outcome of the highest operations of vital force, it would present but a feeble argument for the wisdom of its All-wise Creator.

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ON THE APERTURE OF OBJECT-GLASSES.

I received a note from Mr. R. B. Tolles of Boston, February 17, asking me if I would measure the balsam angle of a $\frac{1}{10}$th objective for him. Having agreed to do so, the objective came to hand before the close of the month. My intention was to measure the angle by the modification of Lister's method proposed by Mr. Wenham,* and afterwards used before a committee of scientific gentlemen in measuring the $\frac{1}{10}$th rashly sent by Mr. Tolles to London for that purpose.† Mr. Tolles, therefore, at my request, supplied a sector and tanks.

Having had some previous experience with the ordinary method of measuring angles of aperture with the sector, I was well aware of the erroneous result likely to be obtained by its use in the case of high angles, but supposed that for the reduced angles to be measured, when the nose of the objective was immersed in water or balsam, it would prove at least as nearly accurate as for similar angles measured in air. I soon found, however, that this was not the case, if the screw collar was fully closed.

I first measured the $\frac{1}{10}$th sent by Mr. Tolles with the screw collar adjusted to the open point, that is, for uncovered objects. The sector, used precisely as described by Mr. Wenham, gave the angle

* 'Monthly Microscopical Journal,' August, 1872, p. 84.
† Ibid, January, 1873, p. 29.

in air at 160°. When the nose of the objective was immersed in a tank of water, the angle was reduced to 93°, and in fluid balsam to 76°, as nearly as could be read by the sector. When, however, the screw collar was adjusted for the thickest cover through which it could work, that is, when the combination was closed as far as possible, I failed to get definite results either in air, water, or balsam. At no angle was the field of view bisected fairly, bright on one side and dark on the other; but the light gradually faded away in such a manner that no sharp limit could be fixed.

I did not feel at liberty to escape this difficulty as Mr. Wenham did, in measuring the Tolles's $\frac{1}{10}$th sent to London, by setting the screw collar at some more open point ("the best adjustment of a Podura scale," for instance), for I had found by trial that when the lens sent to me was closed as far as its screw collar would go, it would still define very well, provided it was used on an object covered by a correspondingly thick covering glass. Worked at this adjustment the lens, in fact, would show the beads of *Pleurosigma angulatum* or the striæ of *Grammatophoro subtilissima* beneath a covering glass one seventy-fifth of an inch thick (by actual measurement). It is fair to say too that this power of working through a thick covering glass with good definition is possessed in a high degree by both the $\frac{1}{18}$th and $\frac{1}{5}$th immersion objectives of Mr. Tolles, which have been described by me in former papers. I note, for instance, that both these glasses will work with good definition through covers of the thickness just mentioned, which none of the $\frac{1}{16}$ths, $\frac{1}{10}$ths, or $\frac{1}{8}$ths, and no other high-angled $\frac{1}{8}$th in the Museum collection will do.*

Having determined, then, that I ought to measure the angle when the combination was closed, and having satisfied myself that the sector was not to be trusted under the circumstances, I devised the following plan, which may be commended for its simplicity and for the definite character of the results.

I had long used an easy mode of measuring the angles of objectives in air, which is, in fact, a modification of the plan of Dr. Robinson, so justly commended by Mr. Wenham.† I screw the

*I may remark here that the thickness of cover through which an objective will work is not limited by its aperture, though this limits the working distance on uncovered objects, but by the extent to which the motion of its posterior combinations neutralize the increasing aberration produced by increasing thickness of cover. The character given to the posterior combinations by the maker determines the available limit in each case.

†'Monthly Microscopical Journal,' November, 1872, p. 233. See also 'Proceedings of the Royal Irish Academy,' vol. vi, p. 38, 1854.

objective into a tube which pierces the shutter of my dark room, the back of the objective being towards the light, and I throw through it, by means of a solar mirror, a parallel pencil of sunlight, which, of course, is brought to a focus in front of the lens and crosses, forming a cone of light. By adjusting a white cardboard protractor horizontally in the middle of the cone with its centre at the visible focus, I measure at once, and without the necessity of any calculation, such as was proposed by Dr. Robinson, the angle of the pencil which crosses at the principal focus; and this angle, as Dr. Robinson has correctly shown, is not materially greater than the angle which would be formed if the light radiated from the conjugate focus used to obtain distinct vision with the eye-piece at the extremity of the microscope body.

To measure the angle in balsam on the same principle, I simply made a thin tank rather more than three inches square, by filling with hot balsam the space between two sheets of plate-glass held about the sixth of an inch apart by narrow strips of glass on three slides. When the balsam had cooled I had, of course, a layer of solid balsam of the size of the tank, with one side open. The tank was carefully levelled horizontally in the cone of light, as the card-board protractor had been, and a drop of fluid balsam on the side where the solid balsam was exposed served to make contact with the face of the lens. When now the solar light was thrown through the lens as before, a superb amber-colored triangle of light started into view, the sharp, well-defined edges of which permitted the angle at the focus to be measured with ease by a card-board protractor held beneath the flat tank, or by any similar device, taking care, of course, that the eye should be perpendicular to the edge of the light-triangles at each reading, to avoid displacement by the refraction of the upper glass of the tank, which would have made a small error. The plan has the advantage that no part of the objective is exposed to the balsam except its face (which is easily cleaned by a little coal oil), besides which the measurements are much more quickly affected than with the sector, and are not liable to the errors which effect its use when the lenses are closed.

By this method, then, I measured the balsam angle of the $\frac{1}{10}$th Mr. Tolles had sent me, with the following results: Uncovered 75°, or nearly what the sector gave; completely closed nearly 80°. I subsequently extended the measurements to the immersion $\frac{1}{15}$th

and $\frac{1}{18}$th by Mr. Tolles, belonging to the Museum, and found that the maximum balsam angle of each was less than 80°. These results, it will be seen, fell within the limits laid down as possible by Mr. Wenham.

To measure the water angle of Mr. Tolles's $\frac{1}{10}$th, I now constructed a thin water tank by cementing strips of glass between the edges of two sheets of plate glass about three inches square, so that they should be held about the sixth of an inch apart. All four sides were closed, but one side had in the centre an opening half an inch long, and the edges of the strips adjoining this were beveled.

When this tank was filled with water, I had of course a thin sheet of water, which would not run out when the tank was held horizontally, and by levelling this, as had been done with the balsam tank, in front of the objective, the angle was measured in the same way. The luminous pencil was by no means so brilliant as in the case of balsam, but its limits were sharp and clear, and it could readily be measured. With the $\frac{1}{10}$th the results were about 90° at the uncovered point, nearly 100° when the objective was corrected for the thickest cover through which it would work. Neither the $\frac{1}{5}$th nor the $\frac{1}{18}$th exceeded 96° when closed as far as possible.

I promptly communicated these results to Mr. Tolles, and was immediately requested by him to examine yet another objective, a $\frac{1}{5}$th, which reached me March 22nd.

On measuring this objective in balsam, precisely as I had done the others, I got somewhat over 90° at the uncovered point, somewhat over 100° when the combination was fully closed. Measured with the water tank, the angle at the uncovered point was about 130°. Now, in the first place, I must remark that the objective was certainly an exceptional one, and apparently put together with a view to this controversy. Instead of three combinations, I found it to be constructed with four; the posterior two resembled those of other fifths of Mr. Tolles, and were together moved by the screw collar, the anterior two remaining stationary; of the anterior combinations the front was very small, and about a ninth of an inch in solar focus. (It magnified 108 diameters at twelve inches' distance from micrometer to screen.) Immediately back of this was a very much larger combination, concave anteriorly and convex posteriorly. I inferred from the manner in which the brasswork was put together,

(having no information from the maker on the subject) that these two combinations had been substituted for the front of a previously constructed objective.

In the next place I must remark that, notwithstanding its exceptional construction, this objective, when used as an immersion glass, had certainly very considerable defining power for a $\frac{1}{5}$th. It worked, it is true, even when fully closed, only through the thinnest covers, but it resolved the *Amphipleura pellucida* and *Frustulia Saxonica*, both mounted in balsam (Möller's type-plate), and on my Nobert's nineteen-band plate clearly separated the lines of the fifteenth band. Used dry it would not work through any cover, but when fully open it resolved the twelfth band of a Nobert's nineteen-band plate, remounted with the lines uppermost and not covered. In this performance the front of the objective appeared to be in actual contact with the object. I may add that the combination when in use magnified at twelve inches' distance sixty diameters at the uncovered point, and seventy-five diameters when fully corrected for cover.

As the results of the measurements of the angle of the objective last described are quite in disaccord with the sweeping opinion expressed by my esteemed friend Mr. Wenham, in his recent controversy with Mr. Tolles, I have thought it right to imitate his prudent example,* and secure the testimony of competent witnesses as to the accuracy of my results. I therefore repeated the measurement of the balsam angle of this objective before Professor Simon Newcomb, of the United States Naval Observatory, and Mr. Renel Keith, of Georgetown, formerly also a professor in the same institution. Both these gentlemen are professional mathematicians, and both are well acquainted with optics as a science. They have not only verified my measurement of the balsam angle of this particular objective, but they agree with me that in the heat of the discussion Mr. Wenham has gone rather too far in concluding that it is theoretically impossible to construct an objective which shall transmit from balsam a pencil greater than 80°.

The position taken by Mr. Wenham is certainly true for objectives as ordinarily constructed ; that it is not necessarily true for all possible constructions will be seen by a moment's reference to his figure.† The deductions drawn from that figure are in strict accord-

* 'Monthly Microscopical Journal,' January, 1873, p. 29.
† Ibid, November, 1872, p. 232.

ance with optical theory only so long as we suppose the lines *d*, *a*, and *b*, *e*, which represent the course of the extreme rays in the crown-glass front of the supposed objective to remain constant. It is not possible for the extreme rays to have greater obliquity if the light passes from air into the glass; but if the radiant is in water and nearer than the point *f*, or in balsam and nearer than the point *g*, it does not follow that the rays cannot enter the glass front, but simply that they will take a course more oblique than the lines *d*, *a*, and *b*, *e*. In the case of balsam of the same index as the glass front there will of course be no refraction at the line of junction between the balsam and the glass, and rays of any degree of obliquity can enter. To what degree of obliquity it will still remain possible for such rays to emerge into air from the posterior hemispherical surface of the front lens, will depend upon the precise form given to it, and how it is possible to collect these rays so as to form an image at the eye-piece will depend upon the construction of the posterior combinations.

In the same way in the excellent paper of the Rev. S. Leslie Brakey, in the March number of the Monthly Microscopical Journal, the conclusions drawn by the author are only true so long as we suppose the direction of the ray O, X, (which precisely corresponds to the line *b*, *e*, in Mr. Wenham's figure) to remain unaltered; the same reasoning applies in both cases. Mr. Brakey remarks that it follows from his demonstration, "that the results are entirely independent of the kind of glass used for the objective front," which is quite true as far as "the results" go, but both he and Mr. Wenham seemed to have overlooked the fact that their demonstrations do not touch the question of the angle possible to be transmitted through an objective from a radiant in water or balsam, but only, to use Mr. Brakey's own accurate expression, the "*reduced angle*" in water or balsam corresponding to a fixed air-angle. Suppose, however, an objective to have such a construction that, when a parallel pencil of solar light is transmitted from behind, the extreme rays shall finally reach the flat surface of the front lens at an angle greater than that formed by the line O, X, in Mr. Brakey's figure, of course if there is air in front of the lens every such ray will suffer total reflexion, while if water or balsam be substituted it will be transmitted.

I am in hopes that the foregoing brief explation will be sufficiently explicit, and that Mr. Wenham himself will frankly admit that he

has overlooked the possible case of an objective made to perform only in water or balsam, without reference to its performance in air. Whether the increased angle which theory demonstrates can be gained at this price, will have any practical value, or be any addition to our optical resources, is another question altogether, and one into which I do not propose to enter at the present time.

Washington. *J. J. Woodward, M. D.*

Note:—

I assisted in the measures above described by Dr. Woodward.

The angle in balsam, when the lenses were fully closed, measured more than 100°.

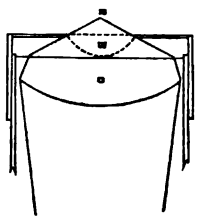
The reason why the angle exceeded the limit laid down by Mr. Wenham was quite obvious to me during the experiments. Whether the objective was open or closed, the light was dispersed in air at all angles up to 180°, showing that the light which struck near the circumference of the anterior surface of the objective must have suffered total reflexion, and so made an angle with the normal to the surface exceeding the limit assumed by Mr. Wenham.

Simon Newcomb, U. S. N.

Washington.

Note:—

I witnessed the measurement, by Dr. Woodward, of the balsam angle of the 1-5th of Mr. Tolles, the method used being that described in the foregoing communication. The angle was over 90° when the lenses were fully open, over 100° when they were fully closed. This result does not seem to me inconsistent with theory. Mr. Wenham's experiments, alluded to in his article in the 'Monthly' for January, indicate an explanation, and it seems singular that they did not suggest to him long ago a method of obtaining what Mr. Tolles has obtained —an objective with large angle for objects covered in balsam. Let O be the lenses of an ordinary objective in adjustment for an object uncovered. Let R be the radiant at such a distance that a cone of large angle is brought to a focus at the eye-piece. In order that this state of things shall not be disturbed, when the object at R is covered in balsam, mount in front of O the lens B, so that when in water-contact with the cover it shall be part of a sphere with its centre at R. It will exactly neutralize the negative surface of the cover, and the light will radiate from R without refraction until it meets the objective at O.



It follows that when a lens of ordinary glass makes balsam-contact with the cover of a balsam-mounted object, the exposed surface of the lens is to be regarded as the *first refracting surface*, and the angle with which a pencil of light may emerge depends upon the curvature of that surface, and has nothing to do with the plane surface of the submerged cover. How much of the pencil may be brought to a focus depends upon the succeeding lenses in the combination. This is strictly true for glass and balsam, having the same refractive index, and is nearly true in all practical cases, even if water be substituted for balsam between the lens and the cover.

Renel Keith.

Georgetown, D. C.

POTATO BLIGHT AND ROT.

SOME eminent chemists, such as Dr. Lyon Playfair, believe that the potato-plant, when healthy, is not subject to attacks from fungi. In a lecture delivered by him before the Royal Agricultural Society of England, December 9, 1845, he remarked that "much had been said and written with regard to the source of the disease, and since minute fungi had been assigned as its cause, potatoes, apples and other fruits had been inoculated with fungus spores, and had become diseased; but if there were not some previous disease in the potato itself, how was it that some varieties of potatoes escaped while growing in the immediate vicinity, while others were attacked?" The disease, he believed, arose from structural or chemical causes.

When a decayed potato was examined it was found that the diseased spots were always in the region of the spiral vessels, whose function it was to carry air into the tissue of the plants. He believed the disease originated in the oxidation of the tissue. The Rev. M. J. Berkley, the leading mycologist of England, on the other hand contends that the fungus *Botrytis infestans*, or, as now classed under the new genus, *Peronospora infestans*, will attack the healthy tubers; but the question arises just at this point, what means have we of ascertaining the perfectly healthy structure and chemical state of tubers? Every farmer plants what he deems sound tubers, yet, in the majority of cases, since 1845, the crop during very moist seasons has been more generally affected than it was prior to that date.

The severity of attacks of fungi on plants will depend in some cases on the density of their organic structure and the solubility of their nitrogenous matter. The nitrogenous principle of potatoes, for example, is soluble in water, that of turnips nearly insoluble. The former, therefore, ferments more readily than the latter. The leaves of a healthy peach-tree, when placed in a moist atmosphere at about 75° F., resist fungoid fermentation for months, while those of a peach-tree affected with the "yellows," placed under the same general conditions, will quickly ferment and become covered with the fruit of the fungus mucor. The first possess an antiseptic property, the second are deficient in it. If two blocks of wood, one of box-wood, the other of pine, are placed in a fungoid solution, the first will resist the action of the mycelium because of its density, while the second will quickly decay. The second absorbs a great deal

of water, the first very little. A certain amount of moisture, and sometimes of water, is necessary to the growth of fungi.

In years previous to the noted potato-rot of 1845, the average amount of water found in healthy potatoes, according to Dr. Playfair, was 72 per cent. That of unhealthy tubers since that date, 80 per cent. The tendency to ferment is therefore increased. It was observed by Dr. Playfair, in his lecture alluded to, that a peculiar state of the weather had been observed all over the north of Europe where the disease had been seen, as well as in America. The widespread use of the potato as an article of diet, especially among the laboring classes throughout Europe, must have led to the extensive planting of diseased potatoes in 1846, because healthy seed could not be found. Indeed, in his second lecture of the 10th of December, 1845, he recommends "the planting of diseased potatoes as seed rather than none." He further states that there was no prospect of obtaining healthy seed from abroad, and that he had permission of the late government authorities for stating that this was the result of their consular returns. The unavoidable adoption of this advice increased the disease in after years, whether it arose from chemical, structural, or fungoid conditions.

If a healthy potato is so dug out on its opposite ends that it will resemble a double egg-cup, and placed erect on one end for about six days in an atmosphere at the temperature of 70° F., its under cavity will become covered with mildew and its fruit will appear in the form of blue mold, *Penicillium glaucum*. In this case the inverted cavity will retain the moisture, and as a consequence slight fermentation will ensue, the fungus deriving its nutriment from the potato; but the upper surface, although fully exposed to the floating germs in the atmosphere, will not sustain a fungus growth, in consequence of the free evaporation of the moisture from it. This form of fermentation should not be confounded with that produced by the fungus of potato-rot, *Peronospora infestans*. The chemical action of the blue-mold fungus is slow, and its odor is simply that of sour paste, while the destructive action of the potato-rot is very rapid, producing a higher state of decomposition and very offensive odors. The mycelium and fruit of each fungus also differ essentially from each other. Both forms of fungus produce oxidation, but with very different results. Consequently potato-rot consists of more than the mere "decay of the tissue by its absorption of oxygen." The

purely fungoid theory, on the other hand, will not account for the many exceptions pointed out by those who favor the chemical theory; since it may be shown that as the chemical constitution and density of any vegetable vary, so will the genus and species of fungi be found to vary with the proximate principles of plants.

The following case of rust on the Kittatinny blackberry illustrates forcibly the fact that the structural and chemical condition of a living plant should always be considered in relation to fungus growth on it.

Chalkley Gillingham, of Accotink, Fairfax County, Virginia, under date of second month 28, 1873, describing the condition of his blackberry canes during the spring of 1872, says that six years ago he planted ten rows of Kittatinny and ten of Wilson in the following manner: First, four rows of Kittatinny, then following, alternately, Wilson and Kittatinny, six rows of each, ending with four rows of Wilson. All had been treated alike from the time they had been received by him, and all appeared healthy until last spring, when the Kittatinny became covered with "rust." At a short distance the rows of Kittatinny appeared as if painted with yellow ochre. Some were destroyed from its effects. None of the Kittatinny canes bore fruit. The Wilson were uninjured, although surrounded by an atmosphere laden with fungus spores. Every leaf of the Kittatinny was covered with hundreds of millions of spores, yet not a leaf of the Wilson was affected. The Wilson canes bore the usual complement of fruit. Mr. Gillingham states that the canes have not been manured for several years.

The glossy covering of fruits and leaves consists of wax, that of the grasses of siliceous matter. The wax may be removed by sulphuric ether, the siliceous matter by caustic alkalies or hydrofluoric acid. Should plants fail to eliminate and cover their surfaces with wax or silica for their protection, their albuminous substances will then afford food for the growth of fungi. Future investigations may prove that in the case of the Kittatiany blackberry alluded to, the absence of this outer protection was the cause of their destruction. I have heretofore noted that two varieties of potatoes (Jackson Whites and Early Rose,) growing in the same field, and treated alike in all respects, were affected differently. The Early Rose potatoes were wholly destroyed by fungi, while the Jackson White, although surrounded by the spores of the potato-rot fungus, were not affected.

Having received a supply of seemingly healthy potatoes from New Mexico, Ohio, and other places, and a few diseased tubors from Boston and Swampscott, Massachusetts, I commenced a series of preliminary experiments to test the chemical and structural theories of Dr. Lyon Playfair, and the fungoid theories of M. J. Berkeley and other leading mycologists.

In four glass jars I placed a pint of water. In No. 1 were placed a portion of fungus *Peronospora infestans*, and the half of an Ohio potato remarkable for its healthy appearance. In No. 2 were placed a diseased potato containing *Peronospora infestans*, and the half of a potato received from Sante Fé, New Mexico. In No. 3 was placed the second half of the Ohio potato alluded to, and in No. 4 the second half of the Santa Fé specimen. In Nos. 3 and 4 was also put half an ounce of pure sugar, to assist fermentation. These specimens were subject, during the experiments, to a temperature of about 75° F. The respective jars were examined from day to day. On the sixth day the Ohio specimen in No. 1 was found to be rotting rapidly, while the Santa Fé specimen in No. 2 was apparently uninjured. Specimens Nos. 3 and 4 were undergoing slow fermentation. At first the water containing the New Mexican specimen became more milky in color than did that of the Ohio specimen, but the deterioration on the third day was greater in No. 3 than it was in No. 4.

On the twentieth day the Ohio specimen was perfectly dissolved, forming a pulp, while the Santa Fé specimen retained its perfect consistency throughout. On examining the pulp of No. 4 under the microscope, I found that the starch granules were arranged in cellulose cells, no liberated granules appearing on the field of view. Bundles of mycelium and budding spores appeared in profusion between the cells. Few infusorials appeared in view. The odor was slightly sour. The appearance of No. 4, as seen under the microscope, of about 80 diameters, was remarkable as contrasted with No. 3. The latter specimens presented a mass of infusorial life, mycelium, and budding spores. I made many examinations of the pulp to detect starch-cells if present, but found none. The fermentation had completely destroyed them. The odor was very bad.

The Ohio specimen in No. 1 rotted much quicker under the influence of *Peronospora infestans* than it did under the *Torula* fungus favored by the action of sugar in No. 4 solution.

The Santa Fé specimen in No. 2 resisted the *Peronospora infestans* fungus better than it did the *Torula* fungus in No. 4; but, by the use of either fungus, the tendency of any variety of the potato to resist fungus action may, by this mode, be easily decided. Since the preceding experiments were made, othern northern and eastern varieties have been tested by fungoid solutions in contrast with some of the New Mexican varieties, giving like results, clearly demonstrating the superiority of the Santa Fé potatoes over all others thus far examined in respect to their powers of resisting fungoid and infusorial action.

It is not unusual to find a decayed spot in the centre of potatoes otherwise apparently in good condition. A microscopic examination of a portion of the diseased part will show that the decay commenced where the vascular bundles concentrate. At that point the air is in greater volume than elsewhere. When such spots are exposed to the atmosphere the fungus, blue mold, forms on the surface. This disease, therefore, has no relation to potato-rot as ordinarily understood.

The vascular bundles are much smaller in some varieties of potatoes than in others, and the texture of the cellular matter varies also. I think it probable that those varieties having the smallest air-passages, all other considerations being equal, will be the least affected by the fungus *Peronospora infestans.*

The air passages of the vascular bundles may be easily seen by the naked eye. Cut a potato in two through its root stem; trim the surface so that some of its eyes will be in section; coat the surface with a solution of bichromate of potash; dry the surface with filtering paper, then coat it all over several times with a strong alcoholic solution of iodine; the starch will become stained of a dark blue, while the vascular bundles will remain yellow. An examination will show that the air-ducts extend in every case to the eyes.

The stalks being simply an extension of the spiral and dotted ducts, it will be seen that any germinal disease entering through the root-stem will necessarily communicate through all the connecting links to the new tuber.

The following mode of separating the vascular bundles from the potatoes, so that they may be viewed separately, will prove of interest to the vegetable physiologist:

Take a potato of medium size, remove the skin carefully without cutting the eyes; place it in a solution of sugar and water, (in the proportion of about two ounces of sugar to a pint of water,) and subject it to 75° F. for about twelve days. The fungus of fermentation will reduce the potato to a pulp, but the vascular bundles will be found apart and may be removed with a glass rod, and mounted in the usual way with gum or balsam. Under a power of about 100 diameters, they present a most singular, yet beautiful appearance. The pointed forms which extend to the eyes may be distinctly seen. This experiment is most successfully performed with the eastern and western potatoes. The starch-cells of the Santa Fé potatoes, for example, remain in a compact form, even when the nitrogenous matter has been destroyed by fungus or infusorials. The "air bundles," therefore, cannot be removed from them easily, which I consider another proof of their matured condition.

The following chemical direct mode of making observations of the position and structure of the air passages I sometimes employ: First, remove from a healthy potato a thin slice or disk. Pour over it concentrated nitric, muriatic, or dilute sulphuric acid, (caustic potash or soda will have the same effect,) when the starch will become transparent. All of these alkalies and acids have the effect of dissolving the starch, but they have no effect on the air-passages, or vascular bundles; they are, therefore, rendered visible, when mounted in the usual manner and viewed by a sufficiently high power.

The Department has procured samples of every variety of potatoes from Santa Fé, New Mexico, to test practically, in the open field, their anti-fungoid qualities, in contrast with the usual varieties grown in that country. In a letter addressed to the Department by Governor Arny, Santa Fé, New Mexico, February 17, 1873, he says: "We have not had any 'rot' in the potatoes of New Mexico. * * * I send two packages of alkali soil; this is a soil on which potatoes will not grow—abundance of tops but no 'tubers.'" An excessively alkaline soil seems to have the same effect as very high manuring, viz.: to produce stalks but no tubers.

Thomas Taylor.

Washington.

ON THE AGENCY OF INSECTS IN OBSTRUCTING EVOLUTION.

SINCE so much has been learned in regard to the agency of insects in the cross fertilization of flowers, I understand the drift of scientific thought to be in the direction of the general principle, that in the hypothesis of evolution, insects play an important part. It does not seem to have occurred to any observer that they may act as an obstruction to any great departure from what we may take as the normal form—that but for them variations would probably often be much greater than they are.

It has fallen to my lot to observe and to place on record in the *Proceedings of the Academy of Natural Sciences of Philadelphia*, the *American Naturalist*, and elsewhere, that art has not so much to do with garden variations as generally supposed; that variations in nature are as great as in horticulture; and that the florist's credit is chiefly due to preserving the form which unassisted nature provided for him. It was at one time part of the essential idea of a species that it would reproduce itself. If any variation occurred in nature, it was taken for granted that seedlings from this variation would revert to the parent form. But it is now known that the most marked peculiarity in variation can be reproduced in the progeny, if care be taken to provide against fertilization by another form. Thus, the blood-leaved variety of the English beech will produce blood-leaved beeches; and, as I have myself found by experiment, the very pendulous weeping peach produces from seed plants as fully characteristic as its parent; and when the double blossomed peaches bear fruit, as they sometimes do, I have it on the authority of a careful friend that the progeny is doubled as its parent was. But I need not refer particularly to this. Any intelligent florist of the present age can testify to the fact, that varieties will reproduce themselves as fully as the original forms from whence they sprung.

I do not think that botanists, as such, are so fully aware of these facts as the florists are. They scarcely admit of much inherent variation in form in nature; but look rather to hybridization, and insect agency in connection therewith, to account for the changes when they occur. In order to avoid the possibility of these agencies acting as the sole factors in evolution, I have generally taken a genus consisting of only one species in a given locality, to show how

great is the variations in form, where no congenital species could mix with it. I have, for this, chosen *Epigæa repens*, *Chrysanthemum leucanthemum*, and the *Quercus neo-mexicana* (*Q. Gunnissonii ?*) of the Rocky Mountains. Another familiar plant to illustrate this is the common yellow toad flax, *Linaria vulgaris*. In a handful of specimens gathered in an afternoon's walk, I find the following variations :—

In regard to the spur, which is generally as long as the main portion of the corolla, some have them only one-third or one-fourth as long; and in one instance the plant bears flowers *entirely spurless*. Dr. James Darrach, a member of the Academy, informs me that he believes he has, in years past, gathered a spurless form, but has neglected to place it on record. Then some plants bear flowers with spurs thick, and others with narrow ones; and while some have spurs quite straight, others curve so as to describe nearly the half of a circle. The lobing of the lower lip is various. In some cases the two lateral ones spread away from the small central one, leaving a free space all around it; at other times they overlap the central one so that it is scarcely seen. Sometimes the small central lobe is nearly wanting—often not more than half the depth of the two large lobes, and at times quite as full, when it may be linear, ovate, or nearly orbicular. The *palate*, as the deep colored process attached to the lower lip may be called, also varies. In color it is pale lemon, but often a brilliant orange. Sometimes it is but about the eighth of an inch in thickness; at others one-fourth, in flowers of the same size. In the case of the shallow flat palate, the attached lobes are patent, or even incurved; while in the thick ones they are very much reflexed. These two forms, when the extremes are selected, are as strikingly distinct as two species often are. Again, the palate is rounded and blunt at the apex; at other times almost wedge-shaped, or at least narrowing to a blunt point. The upper lip varies in proportionate length, sometimes not extending much beyond the palate, sometimes half an inch more; then the margins are sometimes bent down like the wings of a swooping bird; or upwards as in those of a rapidly descending one. Sometimes they are united and turned abruptly up at the apex, like the keel of the garden pea.

And now in regard to the bearing of all these facts on the great scientific questions of the day, we have to note first, that the plant

is an introduced weed, with nothing allied to it anywhere, in the localities where we usually find it, with which it can possibly hybridize. The variations must be from some natural law of evolution inherent in the plant itself. Varieties of course may cross-fertilize as well as species; and some of these variations may be owing to one form fertilizing another form; but there can be no avoiding the fact, that at least the first pair of varying forms must have originated by simple evolution.

Now going back to our florists' experience the question occurs, that as varieties once evolved will reproduce themselves from seed, why does not some one of these *Linarias*, which has been struck off into some distinct mould, reproduce itself from seed, and establish, in a state of nature, a new race, as it would do under the florist's care? Why, for instance, is there not a spurless race? It is scarcely probable that the solitary plant, found on this afternoon's walk, is the only one ever produced. Dr. Darrach's recollection shows it is not a solitary case. The humblebee furnishes the answer. They, so far as I have been able to see, are the only insects which visit these flowers. They seem very fond of them, and enter regularly at the mouth, and stretch down deep into the spur for the sweets gathered there. The pollen is collected on the thorax, and of course is carried to the next flower. The florist, to "fix" the form, carefully isolates the plant; but in the wild state a spurless form has no chance. The bee from the neighboring flower of course fertilizing it with the pollen from any of the other forms.

If there were no bees, no agency whatever for cross fertilization, nothing but the plant's own pollen to depend on, there would undoubtedly be races of this linaria, which, again, by natural evolution at times changing, would produce other races; and in time the difference might be as great as to be even thought generic. But we see that by the agency of the humblebee the progress of the newly evolved form is checked. The pollen of the original form is again introduced to the offspring, and it is brought back at least half a degree to its starting point.

The conclusion seems to me inevitable, that insects in their fertilizing agencies, are not always abettors, but rather at times conservators of advancing evolution.

Thomas Meehan.

Philadelphia.

A CONTRIBUTION TOWARDS A LIST OF RHODE ISLAND DIATOMACEÆ.

In the valuable paper on the Algæ of Rhode Island, by Col. Stephen T. Olney, of Providence, published in the LENS for July, 1872, the references to the Diatomaceæ were chiefly incidental, consisting of a brief list of forms identified by Mr. Thwaites some years since, in the contents of a few bottles gathered in the vicinity of Providence, and sent him by the author of that paper.

The writer having had the opportunity of making gatherings in other parts of the State, in the month of August last, offers the following as a slight contribution towards a complete list of the Diatomaceæ of the State. The sixth column contains Mr. Olney's forms brought opposite my own for convenience. My gatherings contain some forms which I have not yet identified with certainty, and some which I am certain are new, which I propose to describe and illustrate hereafter.

	1	2	3	4	5	6
Achnanthes brevipes, *Ag.*	*	*		*		
" longipes, *Ag.*	*					
" subsessilis, *Kütz*		*				
Actinoptychus undulatus, *Ehr.*	*	*				
Amphora affinis, *Kütz.*	*				*	
" lævissima, *Greg.*					*	
" robusta, *Greg.*					*	
Bacillaria cursoria, *Donkin.*	*					
" paradoxa, *Gmelin.*	*	*			*	
Biddulphia pulchella, *Gray.*					*	
Campylodiscus simulans, *Greg.*	*					
Ceratoneis longissima, *Bréb.*		*				
" lunaris, *Ehr.*			*			
Chætoceros, *n. sp.*, (*description hereafter*,)	*					
Cocconeis dirupta, *Greg.*					*	
" scutellum, *Ehr.*	*	*		*	*	

1. Newport Harbor, near Long Wharf. (128.)
2. Rocky Point, North of Bathing Houses. (110.)
3. Rocky Point, Aquatic Swamp near Forest Circle. (127.)
4. Mark Rock. (129.)
5. Rocky Point, Beach, (131.)
6. Providence, (*S. T. O.*)

	1	2	3	4	5	6
Cocconema cymbiforme, *Ehr.*						*
" lanceolatum, *Ehr.*			*			*
Coscinodiscus lineatus, *Ehr.*		*				
" radiatus, *Ehr.*	*				*	
Cymbella Ehrenbergii, *Kütz.*			*			
Dictyocha aculeata, *Ehr.*	*					
Discoplea sinensis, *Ehr.*	*					
Doryphora amphiceros, *Kütz.*	*					
Epithemia constricta, *W. S.*		*				
" sorex, *Kütz.*					*	
" Westermannii, *Kütz.*	*				*	
Eunotia diadema, *Ehr.*						*
" tetraodon, *Ehr.*			*			*
Fragilaria capucina, *Kütz.*						*
" pectinalis, *Ehr.*						*
" virescens, *Ralfs.*						*
Grammatophora islandica, *Gr.*					*	
" marina, *Kütz.*	*	*			*	
Gomphonema constrictum, *Ehr.*						*
" marinum, *W. S.*	*				*	
" minutum, *Ag.*						*
" truncatum, *Ehr.*						*
Himantidium arcus, *Ehr.*						*
" pectinale, *Kütz.*			*			*
Licmophora pappeana, *Gr.*	*					
Mastogloia Smithii, *Thw.*		*				
Melosira granulata, *Pritch.*	*					
" moniliformis, *Müll.*				*	*	
" nummuloides, *Dillw.*	*	*		*		
" sulcata, *Ehr.*	*					
Meridion circulare, *Ag.*			*			
" constrictum, *Ralfs.*			*			*
Navicula Americana, *Ehr.*			*			
" didyma, *Ehr.*				*		
" elliptica, *Kütz.*			*			
" gracilis, *Ehr.*						*
" lyra, *Ehr.*	*					
Odontidium hiemale, *Kütz.*						*

	1	2	3	4	5	6
Orthosira marina, *W. S.*		*				
Pinnularia gibba, *Ehr.*			*			
" " *forma* gracilis, *Kütz.*			*			
" major, *Rab.*			*			
" mesolepta, *Ehr.*			*			
" peregrina, *Ehr,*					*	
" radiosa, *Kütz.*			*			
" stauroneiformis, *W. S.*			*			
" tabellaria, *Ehr.*			*			
" viridis, *Rab.*			*		*	*
Pleurosigma Balticum, *W. S.*	*					
" elongatum, *W. S.*	*					
" nubecula, *W. S.*	*					
" strigosum, *W. S.*	*	*				
Podosira hormoides, *Kütz.*	*					
Rhabdonema adriaticum, *Kütz.*	*				*	
" arcuatum, *Kütz.*	*	*			*	
Sphenella rostellata, *Kütz*						*
Stauroneis aspera *Ehr.*					*	
" gracilis, *Ehr.*			*			
" Phœnicenteron, *Ehr.*			*			*
Stephanopyxis ferax *Grev.*	*					
Striatella unipunctata, *Ag.*	*					
Syndendrium diadema, *Ehr.*	*					
Synedra crystallina, *Kütz.*					*	
" fulgens, *W. S.*	*				*	
" Gallioni, *Ehr.*					*	
" gracilis, *Kütz.*				*		
" tabulata, *Kütz.*	*	*		*	*	
" ulna, *Ehr.*						*
" undulata, *Bailey.*	*					
Surirella biseriata, *Bréb.*						*
Tabellaria flocculosa, *Kütz.*			*			*
" Thwaitesii, *Olney.*						*

S. A. Briggs.

Chicago, June 15th, 1873.

THE NEW THEORY OF FERMENTATION.

THE indefatigable Pasteur again comes upon the stage with a series of experiments to prove the accuracy of his theory of fermentation. He claims that grape juice, when exposed to the action of the air, or of oxygen, never of itself alone undergoes alcoholic fermentation, but that this only happens when those particles of dust, or germs of ferment, which are present both in the grape and the woody stem are introduced into the must.

The method of experimenting is very simple in theory and perfectly convincing. It is as follows:

Forty glass bulbs were taken, with tubes bent downward to prevent dust falling into them. On the side was a neck fitted with rubber tubing and glass stopper, through which at a given moment the material could be introduced.

These 40 bulbs were filled with an easily fermentescible substance which had been previously boiled, and were divided into four series, of 10 flasks each. Those of the first series contained nothing but the above-mentioned easily fermentescible liquid; the bulbs in the second series had added to this fermentescible liquid a few drops of must or grape juice, taken from the interior of the grape in such a manner as not to come into contact with the dust on the outside of the grape. To the fermentescible liquid in the bulbs of the third series was added a small quantity of the water in which the grapes and stems had been washed and afterwards boiled. To the liquid in three of the fourth series was added some of the water used to wash the grape, and which contained the dust and germs, but had not been boiled. When these preparations were completed, the bulbs were left to themselves and to the action of the surrounding air, in a room of a suitable temperature, or in a bath artificially heated to the temperature most favorable to fermentation.

The result is very surprising, for it was found that the liquid in the first three series, with rare exceptions, had not undergone fermentation; but in the 10 bulbs of the fourth series a very violent fermentation had taken place.

To Pasteur belongs the uncontested honor of being the first to discover that the organisms, in nature, are divided into two classes:

The first class consists of germs visible to the naked eye, and in order to live they require oxygen either free or combined.

The second class embraces microscopic organisms, such as germs of ferment; oxygen acts as a poison on these, but becomes a source of life if derived from a compound like carbonic acid.

It has long been a well-known fact that in fruits taken from the tree and exposed to the air, the vital process goes on in the ordinary manner; they absorb oxygen from the surrounding air and give off carbonic acid. They ripen because the saccharine matter is produced in them without undergoing fermentation.

This premise being established, Pasteur took some fruit, namely, a peach and a plum, and placed them under a bell jar containing carbonic acid; the fruit lost its vitality—its whole life, outer and inner, ceased, because it could not take up and assimilate oxygen from atmosphere surrounding it. The fruit began another and a new life, which developed itself outward from the interior, and is, so to speak, similiar to the life of the atoms, in the sense that the cellular tissue takes away the necessary oxygen from the saccharine matter and other substances present, in the manner of a perfect alcoholic fermentation. The fruit gets soft, it becomes wet through continually, and, if distilled, pure alcohol is obtained and carbonic acid becomes free.

Pasteur repeatedly recurs to these facts, for they are the basis of a discovery of endless importance, and are of greater weight because they will form the connecting link between theories at present opposed to each other.

At the first glance we might suppose that this second discovery was a contradiction of the first, and that the views of Liebig and Fremy—that ferment germs and fermentation itself develop spontaneously in organisms of themselves, without any action from without—were correct; but Pasteur insists that he will soon complete his observations and make all clear.—*Journal Applied Chemistry.*

CULTIVATING WILD-FLOWERS.

BUT few are aware of the many American wild-flowers which merit and would repay cultivation. The showy scarlet sage (*Salvia coccinea*) is a common sea-coast weed in some of the extreme Southern States. In the North it has deservedly become a favorite; and culture has placed it within the reach of every one, even the poorest. The brilliant, deep-red cardinal flower (*Lobelia cardinalis*) is highly esteemed abroad as a garden-plant; and yet, to dwellers in our cities, this plant is almost unknown, although it is one of our common wild-flowers, lavishing its bewitching beauty in numberless places, both North and South. Nor is the above word a mere figure of speech. An English scientific gardener lately visited Long Branch. He took a ride among the surroundings of that watering-place. When between Eatontown and Red Bank, he suddenly requested the driver to stop, at the same time uttering an exclamation which caused Jehu to doubt the gentleman's soundness of mind. The carriage was stopped, and away went the well-dressed Englishman over the field-fence, as lithe and agile as a youth. He actually plunged into the half-swampy ground, and made, as nearly as possible, a straight line towards a scarlet speck in the vernal distance. No high-mettled bull in a Spanish arena ever went more intently at the little red banner of the *picador* than went our friend John B., Esq., through that wet New Jersey meadow for that scarlet flower, which drew him like a fascination. It was a pitiable plight that he presented on his return to the carriage, exultant with his prize. To the astonished driver he offered these apologetic words: "This is the splendid *Lobelia cardinalis*, which I have cultivated with so much care at home, and behold! here it grows wild!" To which Jehu, whose astonishment had now become modified by a shade of contempt, returned an ingenuous equivocation: "That *is* worth a gentleman spoiling his clothes for!"

We know of more than one little cottage flower-patch, whose owner has planted in it the cardinal-flower, where it has grown in such decided prominence of beauty as to maintain a sort of pontifical preëminence among the floral dignities of the parterre. This splendid flower, with its racemes like scarlet rods, and the habit of the plant, so upright and graceful, with a sort of queenly bearing, and

gorgeous magnificence, very much outshone its gayer but straggling companion, the gaudy scarlet salvia. We know a village blacksmith who thus made this plant the spectacle in his flower-pot; and it was amusing to see persons, in their admiration, seeking to purchase plants from this little garden, utterly ignorant of the fact that they could be had simply for the going after in the contiguous meadows. As a wild-flower, they had often seen it, but had never observed it. Forsooth, how few obey the æsthetic command: "*Consider* the lilies of the field, how they grow!"

And there is the common spreading dogbane, to which science has given one of its terrible sesquipedalian names, to wit: *Apocynum androsæmifolium.* It is an engaging plant, for all that, with its open, bell-shaped flowers. Its first cousin, the Indian hemp, though very unpretentious as to its flowers, has an upright habit, much more queenly than the loose *abandon* of its beautiful flowered relation. Alas! for its reputation, this plant has fallen into bad hands, and became notorious among the empirics of medicine. Speaking of the spreading dogbane, a correspondent of the Torrey Botanical Club, quoting authorities, describes it as "one of the most charming of our native plants. The beautiful clusters of rosy bells, with their pink bars, and delicate fragrance, claim for it a place in the garden, where, however, we do not meet with it, but on open banks and by the side of roads or cultivated fields. It is well approved, too, by the insect tribe, who are, in general, much more appreciative judges of color and odor than we are. In Europe, where it is not native, it is cultivated in gardens, and, according to Lamarck, is called *gobe-mouche*—fly-trap. If flies alight on this plant, they are frequently entangled by the glutinous matter, and destroyed. Hence, the plant has been called *Herbe a la puce.*"

It has surprised me that so little has been done with our star-worts, or native astors—plants so prodigal of bloom during the late summer, and almost the entire autumnal months. The number of species is very great, and some are of exquisite beauty. Our favorite is the *Aster concolor.* It abounds South, and comes as far North as the Pines of New Jersey, where it attains perfection in delicacy of structure and prodigality and compactness of bloom. Indeed, this part of New Jersey, has seemed to us as the prodigal borderland, where the Southern and the Northern floras terminate and commingle, or overlap each other. Here Michaux and other great

men have labored, and carried away many novelties. In these regions, the *Aster concolor* grows up like a simple wand, with its small leaves closely hugging the remarkably small stem, much as if a wire had been dressed with leaves for festal uses. The upper part of the stem is so closely surrounded with the compact flowers, that it is literally a purple raceme or wand. Cultivated in mass, in a dry soil, this aster would glow like a sheet of purple flame.

And why is the very common, yet very stately, gentian overlooked? This plant is positively unique in character. A single stem set amid green leaves, with cerulean gems, is a thyrsus worthy of a god. But there is a quaint, coyish modesty about it — its singular flowers seem to be always in bud, as if too coy to blossom outright.

And what charming terrestrial orchids are found native — but, concerning this, there is but space for a word. These singular indigenous flowers — so lovely, and yet so eccentric — are represented by a large number of species. They may be called pretty, winning little oddities. They would need some skill, perhaps, in their cultivation; and some might come to be regarded as the coquettes of the floral community, jilting the gardener with futile promises. Last summer, we took up with our fingers a pretty specimen of the *Calopogon pulchellus*, which means the Beautiful Little Beard. It had but one tiny scape, growing from a green bulb which lay in the moss, much like a solitary egg in a bird's-nest. The entire plant, with its marvellous flower, was not more than six inches high. Our heart failed us in an attempt to put it in the press as a specimen; so we planted it in a little pot, attached to it a label bearing its scientific name, for popular name it had not, and then put it on the glass case on the counter of the apothecary. It was a pleasant surprise to everybody who saw it. Many were the ejaculatory commendations received by the little stranger with the purple hood, and the quaint little beard of so grotesque dyes of pink, and yellow, and white. The pretty stranger was unanimously voted "charming;" and was by some taken to be a rare exotic, that had grown up under the professor's care. Besides this, we have among our native orchids the equally pretty Pogonia and Arethusa; while, worthy of any conservatory, are the white fringed and the yellow fringed Rein-orchis, both of the genus *Habenaria*. Mention might be made of the Lady's Slipper, the showy and rather ostentatious *Cyprepedium*;

but the list is a long one. These native orchids are all eccentricities, and we have selected the most lovable, and the most easy attainable — in fact, those the nearest to our hands.

Just as the above was written, the usual monthly report of the Department of Agriculture came to hand. The following paragraphs are so much to the purpose, that it would be nothing less than blame-worthy not to quote them. Speaking of American plants in Great Britain, it cites an English journal as saying: "The beautiful *Asclepias tuberosa* is, this season, producing freely its showy, bright orange-colored flowers in several collections round London. This fine perennial thrives perfectly well almost anywhere, if planted in sandy peat." In the same journal we find the following: "One of the best hardy aquatic plants, in flower at the present time, is the North American Pickerel-weed (*Pontederia cordata*), a plant by no means so often met with as it deserves to be. It produces a stout spike of handsome sky-blue flowers from 1½ to 2 feet high. No ornamental water should be without this charming aquatic; which should, however, have a place near its margin." "The American Pitcher-plant (*Sarracenia purpurea*) is thriving as well as any native plant in the bog-garden in Messrs. Backhouses' nurseries at York, and by its side a healthy little specimen of the still more curious *Darlingtonia Californica* is beginning to grow freely."

The Asclepias family in America is very rich in species, but the above-mentioned one is by far the noblest of them all. From the fact that it attracts around it large numbers of these beautiful creatures, it is often called the Butterfly-weed. The plant was formerly held in high repute as a medicine, under the name of Pleurisy-root. But its gorgeously-colored flowers, so intensely orange, and so densely massed in heavy umbels, present a gorgeous richness which is incomparable. There is an African species, with flowers of a similar color, which is carefully cultivated in conservatories; but, when contrasted with our native plant, on every count, the foreigner becomes tame, and mean, if not insignificant, in the comparison. As to the Pickerel-weed, it is of easy culture; and in the margin of garden-ponds, or fountain-basins, it might be pronounced as gracefully genteel. The Pitcher-plant, if set higher up on the banks in a bed of sphagnum, or bog-moss, would be so uniquely elegant as to deserve the epithet *recherché*. This same plant can be grown in a pot, simply by keeping the saucer well supplied with water, while

its quaint flowers, and the curious structure of the leaves, would make it the favorite bit of bijoutry in the floral jewels of the window.

This culture of wild-flowers, to some extent, can be indulged in by almost all. Its effect upon a mind of average intelligence is surprising. We have, in our acquaintance, a village bricklayer, a man whose means are of the most slender kind. He has a love for flowers, and shows considerable tact in producing effect by massing the different popular sorts. The imported asters, the improved petunias, and pansies, are severally made to effect a blaze of color. But his chief affection centres in a little spot where he keeps his wild-flowers, among which he pointed out to us, with an amiable pride, his pet pogonias, obtained from the swamp over the way. This man has become quite a systematist in botany, and is deservedly looked upon as the botanical light in his community. And who could possibly indulge in this pleasure of wild-flower culture long without wanting to know the names of his plants? But, as few of them have popular names, he must turn to botany for information. Thus this innocent and elevating pursuit may become a key to the acquisition of scientific knowledge, and the application of scientific methods. Here we stop, with the sense of a child who has picked up a few spangles which have dropped from Flora's rich attire.

Prof. Samuel Lockwood.

Popular Science Monthly.

EDITOR'S TABLE.

WILLIAM S. SULLIVANT, LL. D.,the distinguished Microscopist, and the leading Bryologist of America, died at his home at Columbus, O., after a tedious illness, on the 30th of April last.

He was born January 15, 1803, in Franklinton, a little village literally in the midst of a wilderness, when the present site of Columbus was covered with the primitive forest, and the possibility of such a town not even dreamed of. The early privations inseparable from frontier life strengthened his self-reliance, and developed that muscular strength and activity, united to fine personal appearance and graceful carriage for which he was remarkable, and no doubt laid the foundation of that health and vigor, which seemed but little impaired up to the time of his last and fatal sickness. He accompanied his father on some of his shorter surveying expeditions, where the boy took his first lessons in wood craft, which tended to make him an expert, rapid and accurate surveyor, when, after he had returned from college, he had occasion to exercise his skill in attending to the large landed estate of the family.

He received his early education in Kentucky, fitted for college at the Ohio University, and graduated at Yale college in 1823. Called home by the death of his father in that year, he was more or less occupied with the business of the family estate, instead of studying a profession, as was originally desired by his father. Desiring active employment, he took a position on the surveys of the Ohio canal, and manifested such aptitude and capacity as would have secured him a high position as a civil engineer, had he chosen to adopt that profession. Returning to the old homestead, he took charge of mills belonging to the estate, and having studied and mastered the principles involved in water wheels, mill gearing, &c., he remodeled the mills after plans of his own, and so far as the theory and principles of hydraulics and hydrostatics were concerned, might have found employment, had he so desired, as a master millwright. Henceforward for several years he was actively engaged in business affairs, and became a member of the Ohio Stage Company, whose operations covered a wide field, and before the introduction of railroads, afforded the best accommodations and facilities to the traveling public. Having removed to the country, he improved and adorned that beautiful place now occupied by the Central Ohio Lunatic Asylum. This spot affording unusual facilities for the study of natural history, his attention was turned in this direction, and after devoting some time to ornithology, he finally settled upon botany, influenced in part by his brother, Joseph Sullivant, who was already well skilled in the science, and who found his richest fields in the immediate vicinity of the mansion house, on Sullivant's hill. Suffice it to say, that henceforth for

several years, his leisure was fully absorbed in this attractive field, the first result of which was a well elaborated catalogue of the plants of Franklin county, involving much time and labor. Having thoroughly examined the phenoganmous flora of Central Ohio he longed for "fields and pastures new." Mr. S., fortunately for science, now turned to the study of cryptogamic botany, or rather to the muscological part, wherein he found all he desired for his active and discriminating mind, making many new discoveries and establishing a world-wide reputation in this department; a distinction well deserved and honestly earned by years of quiet but earnest labor. Among his published works are some which are an honor to American science, and a monument of his erudition. To show the extent of his industry and contributions to the science of botany, the following incomplete list is here given:

Catalogue of the plants of Franklin county, Ohio.

Musci Alleghanienses, or specimens of Mosses and Hepaticæ, collected on the Alleghany mountains, 55 sets, each set consisting of two volumes, large quarto, 1845.

Contributions to the Bryology and Hepaticology of North America, with ten plates, quarto.

Mosses and Hepaticæ of the United States East of the Mississippi river, with 8 plates, royal 8vo, 1856.

Mosses and Hepaticæ collected during Whipple's U. S. Government survey for railroad on thirty-fifth parallel to the Pacific, with 10 plates, 4to, 1856.

Mosses brought home by Wilkes's United States Exploring Expedition, 1838–42, with 26 fol. plates, 1859.

Mosses and Hepaticæ collected, mostly in Japan, by Charles Wright, Botanist to Rogers's Northern Pacific Exploring Expedition, with 184 plates, 1860.

Icones Muscorum; or Figures and Descriptions of most of those Mosses peculiar to Eastern North America, which have not been heretofore figured, with 129 copperplates; Cambridge and London, 1864; imp. 8vo.

Papers, chiefly botanical, in the American Journal of Science and Arts, Proceedings American Academy of Arts and Sciences, and London Journal of Botany.

Just before his death he received the proofs of the elaborate engravings illustrating a supplement to the Icones Muscorum he was preparing to publish—and he leaves another work unfinished. A skillful manipulator and expert with the microscope, he had recently prepared a beautiful set of several hundred microscopical slides, containing the dissections of Mosses, and intended as a reference suite.

A member of the American National Academy of Science, and also of some of the oldest and most learned scientific societies of Europe, his labors are better known and appreciated abroad than at home—for his life has been quiet and unostentatious. A ripe classical scholar, he received various titles and degrees, and his works are of standard authority and the highest reputation in Europe and the United States.

In accordance with his wishes his bryological books, and his exceedingly rich and important collections and preparation of Mosses are to be consigned to Harvard University, while the remainder of his botanical library, his choice microscopes and microscopic slides are bequeathed to the State Agricultural College and the Starling Medical College.

JOHN W. FOSTER, LL. D., one of the most eminent citizens of Chicago, died June 29, leaving a blank in the social and scientific world which will not soon be filled. Col. Foster was born in the village of Brimfield, Mass., in 1815, being 58 years of age at the time of his death. Having completed his studies at schools in the vicinity of his home, he entered the Wesleyan University at Middletown, Conn., in 1831, and in due course graduated with high honors. After leaving college, he read law for a year in New England, when he removed to Zanesville, Ohio, and was admitted to the Bar. His education, which was broad and liberal, was the foundation on which he afterwards built an enduring fame. He chose civil engineering as his profession, and began at an early period in his career to follow the natural bent of his genius, and examine cognate sciences, such as geology and metallurgy. On these latter subjects he soon became a recognized authority, in whose judgment Eastern capitalists intending to invest in the mineral lands of the West placed the utmost confidence, sending him to inspect the lands and report upon their resources. In this way he explored a large area of the West, and discovered the archæological remains which opened to him the field of research which he afterwards cultivated with such brilliant success.

The geological survey of the State of Ohio was instituted in 1837, under the direction of Prof. Mathea. He selected Col. Foster, who had been his pupil in former days, as one of his assistants, and the next year he was assigned to an independent district embracing the central portion of the State, and his report embraces a detailed section of the carboniferous limestone near Columbus, to the uppermost bed of coal near Wheeling. This was the first section ever made through the Ohio coal-field.

In 1845, when the copper excitement first broke out in the Lake Superior country, he visited that country in the interest of several mining companies, repeating his visit the succeeding year. The Government instituted a geological survey of the same territory in 1847, under the direction of Dr. Jackson. He was appointed one of the assistants, the labor giving him wide scope for the exercise of his mind in the department of science in which he subsequently became famous. Prof. J. D. Whitney, the noted geologist and metallurgist, of California, was another assistant, and his associate. Two years subsequently the completion of the work was confided to them. The result of their labors was a volume entitled "Foster and Whitney's Report on the Lake Superior Region," which was published by direction of Congress. The report was thorough, and threw much light on the hidden treasures of that territory, which was then a mystery. The work remains the authority on the subject of which it treats, and was one of the most valuable reports ever made to Congress. Messrs. Foster and Whitney made a concurrent report to the American Association for the Advancement of Science, which met at Cincinnati in 1851, and Prof. Agassiz rose at the close of the communication and declared it as among the grandest generalizations ever made in American geology.

While on his various expeditions, Col. Foster collected a vast quantity of material not pertinent to the official report. Explorations in other directions, extending from the lakes to the Pacific, supplemented the fund of information

already in his possession, and enabled him to issue his first important work, "The Mississippi Valley; its Physical Geography, including sketches of the topography, botany, climate, geology, and mineral resources; and of the progress of development in population and material wealth." This comprehensive and interesting work was published in 1869 by S. C. Griggs & Co., of this city, and at the same time was honored with the imprimatur of Trübner & Co., London. It immediately took high rank, both in this country and in Europe, but especially in Europe, where it evoked encomiums from the highest scientific authorities. We have before us one of the volumes, the plates of which were unfortunately consumed in the great fire. It is written in an easy, perspicuous style, and bears evidence of indomitable industry and profound research.

On this work he might have safely rested his reputation, but it was not the crowning work of his life. His specialty was Archæology. In his many wanderings he studied that subject with the devotion of a true scientist, and with a success that few scientists achieve. For twenty years he investigated the strange mounds that are found here and there in the American continent, giving indications of a race of beings anterior to and different from that to which we belong. The significance of these mounds he learned to know, and as scientists in the old world devoted themselves to deciphering the hieroglyphics of the Orient, so he devoted himself to translating the no less hieroglyphical language of these Occidental mounds. His impressions, when first he gazed on these strange symbols of a by-gone age, are beautifully expressed in the introduction to his "Pre-Historic Man." He says:

"In early manhood, when for the first time I gazed upon the works of that mysterious people known as the Mound Builders, my mind received a class of impressions which subsequent years have failed to efface. These works are in the vicinity of Newark, Ohio; and although not the most stupendous, are the most elaborate in the whole series. It was a bright summer's morning, and the sunlight, streaming through the openings of the dense canopy of foliage above, fell upon the ground in flickering patches. A slumbrous silence filled the air, and I confess that, as I traced out the labyrinthine system of earthworks here displayed, with its great circles and squares, its octogons, gateways, parellel roads, and tumuli, the whole spread over an area of several square miles; and as I speculated upon the purposes of their construction, and on the origin and extinction of the people by whom they were used, I was profoundly impressed."

The volume was born as its author died,—it having been issued a few weeks ago. Already it has received merited praise from the American and English press. It is expected to occupy a high place in the library of science. Perhaps it is not inappropriate to note that this work will tend to end the controversy on the Neanderthal skull, about which European scientists have long disputed. Many of them contended that the skull proved nothing regarding a former race of beings inferior to the present, but Dr. Foster had in his possession a skull somewhat similar, but smaller, which is fully described in his last volume. He intended prosecuting his investigations in this interesting domain, the intention being embodied in the conclusion of his preface, in which he wrote:

"If the public manifest sufficient interest in questions relating to our pre-historic archæology to justify the expense, I may hereafter, if life and health are spared, draw more liberally from the materials at my command."

Death came,—to finite minds it would seem too soon,—and his ambition was unfulfilled.

Between the publication of these masterpieces to which we have referred, he was a constant contributor to scientific knowledge in other forms. Among the most notable of his pamphlets were the following:

Description of Samples of Cloth from the Mounds of Ohio. (Transactions of the American Association for the Advancement of Science, 1852.)

On the Antiquity of Man in North America, and Description of Certain Stone and Copper Implements Used by the Mound-Builders. (Transactions of the Chicago Academy of Sciences, 1859.)

On Recent Discoveries in Ethnology as Connected with Geology. An address delivered at Troy, N. Y., as the retiring President of the American Association for the Advancement of Science.

On Certain Peculiarities in the Crania of the Mound-Builders. (Transactions of the American Association for the Advancement of Science, 1872.)

On the Pottery of the Mound-Builders. (*American Naturalist*, February, 1873.)

Dr. Foster also contributed generously to the current literature of the day, and was a regular contributor for many years to *Silliman's Journal*, the *American Naturalist*, the *Lakeside Monthly* and other periodicals.

Dr. Foster was President of the American Association for the Advancement of Science, and has been for three years President of the Chicago Academy of Sciences. He served as Professor of Natural History in the University of Chicago, which honored itself by honoring him with the degree of Doctor of Laws. He made the pursuit of science the object of his life, reaping a large reward of fame. Occasionally he was engaged in service where his wonderful knowledge was invaluable. He was in the Land Department of the Illinois Central Railroad at one time, and reported on the geological formation of the country along the line. He held a like position on the Chicago, Alton & St. Louis Railroad.

Perhaps it was in Europe that the scientific labors of Dr. Foster were most highly prized. His fame had crossed the ocean, and the scientific men of Europe, such as Tyndall, Huxley, Lisle, and Lubbock, were his intimate friends and correspondents. To many a heart in the learned circles of European capitals the news of Dr. Foster's death will bring a pang of grief. His place among the scientific men of this continent, if not of this age, is in the front rank, and his name will stand in history side by side with Agassiz and Maury.

Personally, Dr. Foster was a fine specimen of manhood, strong, finely-built, portly, and handsome-featured. His head bore the stamp of intellect. In manner he was courtly and courteous, genial and kind to all, and it has been said by one who knew him well that he had not an enemy on earth.

PHOTOGRAPHIC REPRODUCTION OF DIFFRACTION GRATINGS.—Experiments made by Hon. J. W. Strutt, some months since, with a view to the production of photographic copies of diffraction gratings ruled upon glass, have given interesting

and valuable results, of which he gave an account in a communication read before the Royal Society, June 20, 1872. The account is republished in the *Philosophical Magazine*. The ruled plates were laid upon glass plates sensitized in the usual manner, and the prints were made in the same manner as from ordinary negatives. Both wet and dry sensitive plates were used, with but little difference in the results. The photographic gratings have brilliant spectra, and were but little inferior to those ruled upon glass. In the course of the experiments, trial was made of plates covered with a film of bichromatized gelatine. The gratings thus made possessed a high degree of transparency, and were found to be better than the ordinary photographs; and although there was some uncertainty attending their production, the best obtained appeared to be even superior to the originals on glass. They give very brilliant spectra, and the definition of the lines is surprisingly good. They can be used very conviently in an ordinary spectroscope, by putting them in the place of the prism. Gratings having 6,000 lines to the inch are now successfully made; and as their cost is trifling compared with that of the ruled ones, they will be much more accessible to experimenters. As the thickness of the glass upon which they are mounted is small, the absorption of the rays is very slight, and they offer considerable advantages in researches upon radiant heat, as they may replace to a large extent the costly and inconvenient prism of rocksalt.

A NEW METHOD OF VIEWING THE CHROMOSPHERE.—A paper on this subject was recently read before the Royal Society by J. N. Lockyer and G. M. Seabroke. An artificial eclipse is produced by covering the sun's disk by a disk of brass. It is, in fact, the replacement of the moon by another sphere or semisphere (or rather a disk, in this method). The idea occurred to both authors at different times. The image of the sun is formed on a diaphragm, having a circular disk of brass (in the center) of the same size as the sun's image, so that the sun's light is allowed to pass. The chromosphere is afterwards brought to a focus again at the position usually occupied by the slit of the spectroscope, and in the eyepiece is seen the chromosphere in circles corresponding to the C and other lines. A certain lens is used to reduce the size of the sun's image and keep it of the same size as the diaphragm at different times of the year; and other lenses are used to reduce the size of the annulus of light to about $\frac{1}{8}$ inch, so that the pencils of light from either side of the annulus may not be too divergent to pass through the prisms at the same time, and that the whole annulus may be seen at the same time. There are mechanical difficulties in producing a perfect annulus of the required size, so one $\frac{1}{2}$ inch diameter is used, which can be reduced virtually to any size at pleasure. The proposed photographic arrangements are as follows: A large Steinheil spectroscope is used, its usual slit being replaced by the ring one. A solar beam is thrown along the axis of the collimator by a heliostat, and the sun's image is focussed on the ring slit by a $3\frac{1}{4}$ inch object glass, the solar image being made to fit the slit by a suitable lens. By this method the image of the chromosphere received on the photographic plate can be obtained of a convenient size, as a telescope of any dimensions may be used for focussing the parallel beam which passes through the prisms on to the plate.

NOBERT'S NEW TWENTY BAND TEST-PLATE.—A recent number of the *Scientific American* contains a description of a new twenty band plate, the property of Dr. F. A. P. Barnard, of Columbia College, New York, which surpasses in fineness any of M. Nobert's previous productions, the bands ranging from three thousand to two hundred and forty thousand lines per Paris inch. The description of the Plate having been copied in *Cap and Gown*, and containing some inaccuracies, Dr. Barnard has sent the latter journal a communication correcting the same, which we have thought might be of interest to our readers. Dr. Barnard's Test-Plate, however, is no longer unique, Dr. J. J. Woodward, of Washington, being the lucky possessor of a Plate, which, though covering the same range, has the advantage of perfection in the higher bands, which when Dr. Barnard wrote his communication he supposed to be an impossibility.

The following is the letter of Dr. Barnard:

I perceive that you have transferred to your columns the brief notice which recently appeared in the *Scientific American*, of the Nobert Test-Plate received by me a few months since, which, for the extreme fineness of its divisions, is, I believe, unique, at least for the present, in this country. The description is substantially correct, though embracing one or two unimportant inaccuracies to be mentioned below; but it represents Mr. Nobert as having said something which he did not say, and which he is not likely to say, viz.: that he would undertake to rule finer lines in case these should be resolved. In 1868, in sending out to me one of his nineteen band plates, of which there are now so many in the country, and when the divisions of those plates had not yet been optically made out, he did say that if the microscopists should make out those lines, he would rule finer ones. The nineteen band plate has been perfectly resolved; and the proof is not only optical but photographic, Col. Dr. J. J. Woodward, of the Army Medical Museum at Washington, having made prints of even the nineteenth band, in which the divisions number one hundred and twenty thousand to the Paris inch (say one hundred and thirteen thousand to the British,) which present the separate lines distinctly to the eye. Mr. Nobert has, therefore, been as good as his word in ruling finer lines; but he has only obtained the great success of doubling the largest number previously ruled in a given space, after many trials which resulted in failure. My opinion is that he has reached the limit of human skill in this direction, and that he will not attempt to surpass his present achievement. He says indeed that he is so overburthened with orders of various kinds at this time, that he would not undertake to rule another plate similar to this one for some months. By experience I know that his months are long ones. Of course he cannot, if he would, soon attempt to do more.

This plate has not yet been fully resolved, either optically or photographically. I question whether it ever will be so, throughout its whole extent.

The points in which the description copied by you from the *Scientific American* are not quite exact are the following: It is stated that the plate is of glass, three and a half inches long. The glass, on which the ruling is done, is a circle, about half an inch in diameter, and not more than one two-hundredth or one three-hundredth of an inch in thickness. This is cemented by means of Canada balsam, applied near the circumference, the ruled side being downward, to a similar disk about one one-hundredth of an inch in thickness. The extreme thinness of these plates is designed to admit of the use of the highest magnifying powers of the microscope, in which the front of the object-glass is brought almost into contact with the object viewed; and also to allow the illuminating condenser, by means of which light is concentrated on the object, to be brought almost equally near on the other side.

The compound disk above described is mounted in a brass plate, which has about the dimensions stated in the article quoted by you, viz.: three and a half

inches by one inch. But, in point of fact, the brass plate contains not one but two such compound disks. In ruling the higher bands, Mr. Nobert finds that it is impossible to make every band in every plate quite perfect. Here and there a fault will occur from the lines running together, or rather from the splintering of the glass between the lines. He accordingly mounts two plates in the same setting, so selected that the same bands shall not be faulty in both. The faults are not many in the plates in my possession; and none of the faulty bands appear to be faulty, except in small portions of their length.

As yet, since receiving this plate, the pressure of my occupations has not permitted me to test upon it, to my satisfaction, the highest microscopic powers at my command; and I cannot tell how far it is optically resolvable. I should very much like to have it examined by the photographic method, by an expert so able as Col. Dr. Woodward.

F. A. P. B.

PHYSIOLOGY OF A SPONGE.—Rev. Samuel Lockwood publishes in the last number of the *Popular Science Monthly*, a most interesting and highly instructive paper on the Glass Sponges. Of the physiology of a sponge, he says:

"If we take a morsel of a toilet-sponge, and put it under a microscope of moderate power, we find that it is made up of a mass of complicated net-work. There is more or less regularity in the meshes; and these are found of various patterns in the different species. This heap or mass of net-work, commonly called a sponge, is really the skeleton of the sponge. When living it is covered with, or literally embedded in, a glairy, gelatinous, or albuminous substance. But this is so unlike ordinary animal tissue—for it seems, really, tissueless—that it has received the technical name *sarcode*. This sarcode fills the meshes above mentioned, and is held in place by innumerable tiny spicules, mixed in, so to speak, like the hair in the mortar of the plasterer. So little consistency has this sarcode, or sponge-flesh, that but for this natural felting it would dissolve and flow away. Now, take an ordinary sponge into the hand. We observe several large apertures, at or toward the top. These are called the *oscula*. They are the exhalent vents of the entire system. At these openings is expelled, with some force, the water that has been taken into the living mass, and deprived of its nourishment. But how is the water brought in through that glairy sarcode? Besides the oscula, which are few, and readily seen, even in the skeleton, there are innumerable tiny inlets known as *pores*. These are not visible in the skeleton, as they really belong to the sponge-flesh. These pores open into the meshes, and enter certain little cavities, or chambers, that stand connected with circuitous passages, which finally lead to the large outlets, or oscula. The pores are very small, and yet, compared with the cells, are very large. The little chamber into which the pore opens has its walls built up with these uniciliated cells. Now, if we could peep into the privacy of that chamber, with its walls of living stones, without making any disturbance, we should find every cell lashing its cilium with great vigor, and all in such harmony of accord, that it would seem like

> "Beating time, time, time,
> In a sort of Runic rhyme."

"The beating of each lash is doubtless downward, that is, inward; the effect of which is, a *vacuum* above, into which the water presses through the external pore. A second result of this downward beating of the cilia from a myriad of

cells is, the impulsion of the passing water through the ramifications leading to the oscula. Thus the running of the waters is the sponge's ancient 'Runic rhyme.' Every sponge, then, has a very complete aquiferous system: its conduits at the entrance of and along which the busy one-lashed cells occupy themselves forcing the water along; and the oscula, which may be likened to the outlets of sewers. During this circulation of the fluid through the living mass, the sarcode obtains its nourishment, and the skeleton its growth by a sort of absorption, or what is known to the physiologist as endosmotic action of the cells. We have then mentioned above three clearly specialized functions, as represented respectively by the inhalent pores, the exhalent oscula, and the uniciliated cells. And it is certainly a matter of prime importance that each cell should have this single lash. In fact, it raises it to the rank of a pacha with one tail, in a community where all are pachas of this dignity, and each one a commissioner of the water department, and a commissary of subsistance. 'Both the oscula and pores can be closed at the will of the animal; but the oscula are permanent apertures; whereas the pores are not constant, but can be formed afresh whenever and wherever required.' "

THE EUPLECTELLA SPECIOSA.—From the same paper we extract this graphic description of this wonderful anchoring sponge:

"It is almost hopeless to attempt a description of *Euplectella* in words. Nor has any artist yet done justice with his pencil to the delicate fabric. The first specimen that reached England, and which for a long time was the only one known, was purchased by William J. Broderip, for the sum of $150 in gold. Says Prof. Owen: 'Mr. Cuming has intrusted to me for description, one of the most singular and beautiful, as well as the rarest of the marine productions.' *Euplectella* is in form a cornucopia, at the lower end about an inch in diameter, and in good specimens, after making a graceful curve, terminating at top in a width of nearly two inches. This part has a cover with a frilled edge, which, in a complete specimen, projects about a fourth of an inch over the sides. The bottom, or smaller end, is encompassed with a dense ruff of glass threads, so delicately white, flexible, and fine, that they look like a tuft of floss-silk. This muff-like surrounding is sunk into the deep-sea ooze, the fibres pointing up, which, though effectual, is certainly an odd way of mooring itself. In this manner this sponge is, when living, in a perpetual bath of mud. Like *Hyalonema*, our *Euplectella* is an anchoring sponge. Venus's Flower-Basket looks like a structure made of spun-glass; and so fragile that one hesitates to take it into the hands. It is wonderfully light—reminding, in this respect, of the skeleton or phantom flowers sometimes seen under glass. But *Euplectella*, although really so delicate, is quite strong. The threads which make up this fabric of woven glass are so flexible that a body is led to wonder if this is like the product of that lost art. To us it seems doubtful whether any woven glass, the product of art, can quite affect the singular lustre that belongs to these silicious threads spun from Nature's distaff. Each thread, although of pure silica, and solid, is really composed of series of concentric tubes or cylinders, as if spun on a central thread or core. The effect, as

respects the light, is not easily described. As the threads are composed of pure silica, one might suppose that they would be transparent, as a film of pure white glass of equal thickness. Such is not the fact. They are translucent, and have just an appreciable tint of the opal. It is this that imparts to *Euplectella* that softness of aspect which has been called 'a delicate satiny lustre.' To us the term opalescent seems better. We have a specimen which, in a good light, shows the play of colors that frozen crispy snow does in the moonlight.

"As to the idea 'well woven,' which the name contains, the fabric really seems to have its web and its woof. There are long threads that traverse the whole length; and there are others that cross and interlace, or, more correctly, interweave. And, what no loom of human invention has ever done, this lowly weaver makes the fabric as it progresses, take on the most quaintly little flounces with the most delicate frilled edges imaginable; and all arranged in such charming grace and ease—not in parallel circles, like hoops on a barrel, but in tasteful, easy-flowing curves. In the configuration of the innumerable forms of structure, Nature, as she ascends in the grade of her work, almost abandons her parallels in in the outlining and ornamentation of her constituted things. In the mineral province the structure of crystals shows her delight in parallel, straight liñes. The curve is a rarity there. But in organic forms the curve is the rule, and the straight line is the exception. The lace-like structure of the *Euplectella* is so aërial a fabric, and so quaintly graceful, and, as one might say, so deftly done in the putting together, that any embroidery would seem in the comparison bungling. Enflounced in its own tiny, crispy frills, there is an air of improvised beauty. And there is a flavor of rank in the almost grotesque hint thrown out by the sometimes queer sort of relief afforded in this excess of elegance by a dash of chevron-work.

THE LIMITS OF MULTIPLICATION.—In Herbert Spencer's paper on the study of Sociology, in the current issue of the *Popular Science Monthly*, he thus defines the limits of multiplication:

"Every species of creature goes on multiplying till it reaches the limit at which its mortality from all causes balances its fertility. Diminish its mortality, by removing or mitigating any one of these causes, and inevitably its numbers increase until mortality and fertility are again in equilibrium. However many injurious influences are taken away, the same thing holds, for the reason that the remaining injurious influences grow more intense. Either the pressure on the means of subsistence becomes greater; or some enemy of the species, multiplying in proportion to the abundance of its prey, becomes more destructive; or some disease, encouraged by greater proximity, becomes more prevalent. This general truth, everywhere exemplified among inferior races of beings, holds of the human race. True, it is in this case variously traversed and obscured. By emigration, the limits against which population continually presses are partially evaded: by improvements in production, they are continually removed further away; and along with increase of knowledge, there comes an avoidance of detrimental agencies. Still, these are but qualifications of an inevitable action and reaction."

THE DIVISIBILITY OF MATTER.—The same Journal publishes a translation from the *Revue des Deux Mondes*, of an essay by Fernand Papillon on the constitution of matter, in which occurs the following illustration of divisibility:

"Let us dissolve a gramme of resin in a hundred times its weight of alcohol, then pour the clear solution into a large flask full of pure water, and shake it briskly. The resin is precipitated in the form of an impalpable and invisible powder, which does not perceptibly cloud the fluid. If, now, we place a black surface behind the flask, and let the light strike it either from above or in front, the liquid appears sky-blue. Yet, if this mixture of water and alcohol filled with resinous dust is examined with the strongest microscope, nothing is seen. The size of the grains of this dust is much less than the ten-thousandth part of 1-25 of an inch. Moren makes another experiment, proving in a still more surprising way the extreme divisibility of matter: Sulphur and oxygen form a close combination, called by chemists sulphuric-acid gas. It is that colorless and suffocating vapor thrown off when a sulphur match is burned. Moren confines a certain quantity of this gas in a receiver, places the whole in a dark medium, and sends a bright ray of light through it. At first nothing is visible. But very soon in the path of the luminous ray we perceive a delicate blue color. It is because the gas is decomposed by the luminous waves, and the invisible particles of sulphur set free decompose the light in turn. The blue of the vapor deepens, then it turns whitish, and at last a white cloud is produced. The particles composing this cloud are still each by itself invisible, even under strong microscopes, and yet they are infinitely more coarse than the primitive atoms that occasioned the sky-blue tint at first seen in the receiver. In this experiment we pass in steady progress from the free atom of sulphur parted from the oxygen-atom by the ether-waves to a mass apparent to the senses; but if this mass is made up of free molecules which defy the strongest magnifiers, what must be the particles which have produced those very molecules!"

THE FOVILLA OF POLLEN.—Signor Saccardo states in the *Nuovo Giornale Botan. Ital.*, that botanists are agreed that the minute grains in the contents of pollen consist of starch-granules, oil-globules, sugar and nitrogenized compounds, but, so far as he is aware, no observer has yet noticed among them certain minute bodies of well-marked and constant shapes. He detected, in June last, very small oscillating bodies which make up the bulk of the fovilla, and to these he gives the name *Somatia*. The form of the somatia is invariable in the same species of plants, and in plants of the same genus the forms appear to be nearly identical. The plants most carefully studied were *Cucurbita Pepo*, *Eschscholtzia crocata*, *Onagraceæ*, *Portulaca grandiflora*, *Althæa rosea*, whose *somatia* are figured as fusiform, discoid, etc. To observe these small bodies to the best advantage the author advises that a drop of distilled water should be placed on a few grains of the pollen on a slide, and then the cover should be pressed down so as to crush them. The somatia are seen under a magnifying power of 800 to 1,000 diameters to have an oscillating motion which may be referred to the "Brownian movement." Treated with a solution of iodine, the color of these somatia becomes blue; but this tint is marked only in the central portion, while the outer part remains clear.

THE EYES IN DEEP-SEA CREATURES.—In his *Notes from the Challenger*, Wyville Thomson says: The absence of eyes in many deep-sea animals and their full development in others is very remarkable. I have mentioned the case of one of the stalk-eyed crustaceans, *Ethusa granulata*, in which well-developed eyes are present in examples from shallow water. In deeper water, from 110 to 370 fathoms, eye-stalks are present, but the animal is apparently blind, the eyes being replaced by rounded, calcareous terminations to the stalks. In examples from 500 to 700 fathoms, in another locality, the eye-stalks have lost their special character, have become fixed, and their terminations combine into a strong, pointed rostrum. In this case we have a gradual modification, depending apparently upon the gradual diminution and final disappearance of solar light. On the other hand *Munida*, from equal depths, has its eyes unusually developed, and apparently of great delicacy. Is it possible that in certain cases, as the sun's light diminishes, the power of vision becomes more acute, while at length the eye becomes susceptible of the stimulus of the fainter light of phosphorescence?

DOUBLE FERTILIZATION OF FEMALE FLOWERS—Mr. Arnold, of Paris, Canada, has shown that if the female flowers of an Indian-corn plant are submitted to the action of pollen from male flowers of different kinds of corn-plants, each grain of the ear produced shows the effect of both kinds of pollen. In an experiment related, a given female flower was subjected first to the action of pollen from a yellow variety of corn, and then to that taken from a white variety; the result was an ear of corn each grain of which was yellow below and white above. The conclusion presented is, not only that there is an immediate influence on the seed and the whole fruit-structure, by the application of strange pollen, but the more important fact that one ovule can be effected by the pollen of two distinct parents, and this too, after some time had elapsed between the first and the second impregnation.

FERN PRESSING.—Under the caption *Home and Society*, in the last number of *Scribner's Monthly*, we find the following:

"The girls should not forget that this is the time to gather and press ferns. They are so pretty and refreshing to have in the house in cold weather, so easily obtained, and so little trouble to prepare, that it is a pity any one should be without a few bunches when the flower-season has passed. There are many modes of preserving them; but the one that seems most successful is to pick the ferns when they are young and tender; lay them between newspapers, or in large, flat books, and place them under very heavy weights, until the sap has entirely dried. Persons who gather them in August, often leave them in press till Thanksgiving or Christmas; asserting that this long subjection to the weights keeps the color better than any other method. The safest way to secure perfect ferns is to take a book to the woods, and lay each one between the leaves as soon as broken from the stem. Even in a few minutes, ferns will curl at their tips, and after an hour or two, it is almost impossible to lay them flat. This process is very good for bright leaves, and makes them look less artificial than when they are varnished.

Bunches of Autumn leaves are very beautiful evening decorations, if a lighted candle be set behind them. This brings out their brilliant tints, and gives them the appearance of having been freshly gathered."

NERVATION OF THE COATS OF OVULES AND SEEDS.—A brief article by Van Tieghem in *Comptes Rendus*, and *Ann. Sci. Nat.*, and a long one in the latter journal by LeMonnier (apparently Van Tieghem's pupil), develop clearly the former's view respecting the morphological nature of the ovule. He deduces the foliar nature of its envelope from its "libero-vascular system," which is that of the leaf. It answers, as has been before explained, to a marginal lobe of a carpellary leaf transformed and convolute around the nucleus, which, being destitute of vascular tissue, is a "parenchymatous excrescence," a *trichome*, to use the recent term of the Germans. LeMonnier sums up the conclusions thus: 1. The ovule always consists of a lobe of a carpellary leaf, folded around a cellular *mamelon* inserted upon the medial line of the lobe; 2. In Angiosperms upon the upper or *trachean* face of the leaf; in Gymnosperms upon the lower or *liberian* face. 3. The embryo, although discontinuous from the tissues of the mother-plant, has determinate relations of position; not only is the radicular extremity always directed to the micropyle, but its principal plane is generally perpendicular to or parallel with that of the seminal lobe. 4. The primine, characterized by the presence of vascular bundles, is commonly the only membrane which persists in the mature seed; the secundine, except in rare cases (*Euphorbiaceæ*), is only a deduplication of the primine, and is mostly transitory.

THE STRUCTURE OF THE CYSTIDIA.—This is discussed very fully in Mr. Cooke's *Grevillea*, in a translated paper by M. A. de Bary. The structure of the cystidia, he says, offers a few peculiarities; in the greater part a delicate and colorless membrane surrounds sometimes a similarly colorless plasma, full of vacuoles, and sometimes a perfectly transparent liquid. He has observed in the hymenium of *Coprinus micaceus* which had not yet attained its maturity, that the cystidia enclosed a central plastic body, irregularly elongated, which sent in all directions towards the sides of the cell a multitude of filiform processes, branching and anastomosing amongst themselves. These processes changed their form with astonishing rapidity, after the manner of the *Amœbæ*. The older cystidia were entirely transparent. The contents of the cystidia of *Lactarius deliciosus*, and allied species, are granular and opaque. In this respect the cystidia resemble the lactiferous tubes or filaments, and often when a thick slice of the substance of the fungus is observed, it seems that they are branches from these filaments, the more so since they bury themselves deeply in the weft of the lamellæ, underneath the subhymeneal tissue. Still he has never seen them spring except from filaments of the weft deprived of latex, of which they seemed to be branches. The cystidia of *Agaricus balaninus*, Berk., are of a dark purple color. According to Corda and the uncertain opinions of anterior authors, the cystidia eject their contents under the form of a liquid drop, and that by their summit, which is represented as open. He has not, any more than M. Hoffman, been able to convince myself that this phenomena is produced spontaneously. He has, indeed, only very rarely

seen the cystidia burst in the water, which the same author says takes place very irregularly. If their surface is damp, and often bears liquid drops, this is a circumstance which is common to them with all fungoid cells that are full of juice.

WHAT ARE INSTINCTIVE ACTIONS?—This is really, to the thoughtful man, who is learned on the subject, the most intensely difficult question. A paper on this question appears from the pen of Mr. George Henry Lewis, in *Nature*, and is well worthy of perusal. The author states, among other things, that the fact that we require some character to distinguish the instinctive from the impulsive actions, may be readily shown. No one calls breathing, secretion, excretion, &c., instincts. Yet these are the actions of congenital tendencies in the organism. "A hungry chick," says Mr. Spalding, "that never tasted food, is able, on seeing a fly or spider for the first time, to bring into action muscles that never were so exercised before, and to perform a series of delicately adjusted movements that end in the capture of the insect." Every one would pronounce this a typical case of instinct. Now compare with it the following, which no one would class among the instincts: A new-born animal that has never breathed before is able, on first feeling the stimulus of the atmosphere, to bring into action a very complicated group of muscles which were never so exercised before, and to perform a series of delicately adjusted movements which end in the aëration and circulation of the blood. This contrast may lead us to the character sought. Understanding that every line of demarcation in psychical phenomena must be more or less arbitrary, and only justified by its convenience, we may draw such a line between impulse and instinct. Impulses are the actions which from the first were fatal, inevitable, being simply the direct reflex of the stimulated organs. Given the respiratory organs and the atmosphere, respiration is the inevitable result. Given the secretory organ and the psalma, secretion is the inevitable result. There is no choice, the action either takes place or it does not.

ANALYSIS OF THE AIR OF PUBLIC SCHOOLS.—The *Sanitarian* says that from the public schools of New York, Dr. Endemann obtained seventeen samples of air, the examination of which determined the presence of carbonic acid, varying in amounts from 9.7 to 35.7 parts in 10,000; or in other words, from more than twice to nearly nine times the normal quantity. The ventilation in these buildings is generally faulty, and can be obtained only by opening the windows—a practice detrimental to the health of the children who sit near or directly under them. The following experiment, made in the Roosevelt Street School, shows the inefficiency of ventilating flues in the wall unprovided with means for creating an upward current. An examination of the air in one of the class-rooms provided with a ventilating flue was made while one of the windows was opened, and yielded 17.2 parts of carbonic acid in 10,000. The window was then closed, and after the lapse of ten minutes another examination gave 32.2 parts of carbonic acid, or an increase of 15.6 parts. The experiment now became to the teacher and children so oppressive that it was not continued. Dr. Endemann says: "If the accumulation of carbonic acid had been allowed to continue, we might have reached within one hour the abominable figure of 110."



Woodburytype. A. P. B. P. Co., Phila.

FIRST SIX BANDS OF NOBERT'S PLATE.

WEBB'S TEST, "THE LORD'S PRAYER."

EACH MAGNIFIED 640 DIAMETERS.

From Photo-Micrographs by Dr. J. J. WOODWARD, U. S. A.

THE LENS;

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THE GERM THEORY AND ITS RELATIONS TO HYGIENE.

THE germ theory of disease is not, as is commonly supposed, a theory which has originated in very recent years. More than two hundred years ago it was brought forward, at least as a hypothesis, by the celebrated Father Kircher, in his *Scrutinium physico-medicum contagiosæ luis quæ pestis dicitur*, to account for the infectious propagation of the plague. However plausible this theory might at the time have seemed, it could then, nevertheless, claim no higher rank than that of a bare hypothesis; and it has only been in times comparatively recent that observation has brought to light a sufficient number of facts apparently favoring it to justify our advancing it in the arena of scientific discussion to the higher dignity of a theory.

Before proceeding to consider the evidence bearing on the truth of this theory, for or against, a few observations of a general nature may properly here find place. No living organism enjoys an existence of unlimited duration. Every such organism, under favorable circumstances, passes through three distinct stages, which are those of growth, vigorous maturity, and decline. The organism commences as a germ, and ends in dissolution and disintegration. Since the laws of life, as well as those of physics, are fixed and definite, there is reason to believe that all organisms of the same species,

if placed in conditions equally favorable to their development, would be equally long-lived; yet, in point of fact, those which pass through the regular stages constituting their normal life are comparatively few. In the large majority, the vital functions are, earlier or later, more or less disturbed, if not arrested, by an endless variety of causes tending to produce disease and premature death. In the human race, life is often shortened by ignorant or wilful disregard of the conditions necessary to the preservation of health. Accident, also, often exposes individuals to deleterious influences. Thus, in many cases, diseases arise from exposure to extremes of temperature, or from excesses in eating and drinking, persisted in until the organs of digestion become debilitated and fail to fulfil their proper functions. But beside these causes of disease, which may be classed under the head of "injurious conditions," there are other influences directly morbific, which, whenever they come into play, cut short the duration of life. Poisons belong to this class, but the effects of these are felt only in occasional and accidental instances. Other noxious influences, of which the pernicious consequences are more widely spread, are those which produce the diseases called zymotic. Such are malaria, contagion and infection, instrumentalities to which are owing the widespread ravages of epidemic.

It may be remarked that there are many cases of disease in which the cause is not traceable directly to any of the sources above mentioned, but in which the disease has been transmitted by inheritance from a parent similarly affected. In such cases there is nevertheless every reason to believe that the disease in its first appearance was produced in a healthy organism by causes belonging to one or the other of the classes above named.

The diseases which it is the object of the present paper to consider are only those which belong to the epidemic or contagious class.

No subject has occupied more the careful attention of physicians, or has been a subject of more elaborate observation and experiment, or has led to more marked difference of opinion or more animated controversy, than that of the nature of the influences by which these diseases are transmitted from individual to individual. That many epidemics arise from peculiar conditions of the atmosphere, not in the least as yet understood, can hardly be doubted;

and in this case the influence which excites disease simultaneously in many is not dissimilar to that by which contagious diseases are transmitted from individual to individuals. Two theories, distinctly opposed to each other, have long been held on the subject. These may be distinguished as the chemical theory of infection and the germ theory. The chemical theory is founded on a presumed analogy between the propagation of disease in living organisms and the process of fermentation in certain forms of organic matter without life. This theory assumes a ferment to be an organized substance in a certain state of decay, which possesses the property of exciting the same decay in other organic substance with which it is in contact. Applying this theory to disease, it supposes that infection is communicated by the instrumentality of particles thrown from the person, or from substances proceeding from the person diseased, and borne by the air to other persons in full health, in whom they excite, probably by contact with the membranous linings of the lungs, the same diseased condition which exists in the patient. The opposing theory presumes that the diseased person is suffering from an invasion of his system by microscopic algoid or fungoid vegetative forms having the property of rapid self-multiplication, and that the spores which proceed from these fungi or the cells of the algæ are wafted in like manner by the air from person to person, penetrating the systems of the healthy, and establishing new colonies to generate disease in them.

A *prima facie* evidence, which so far as it goes is favorable to the germ theory is found in the well known fact that all the forms of cryptogamic vegetation are propagated by spores, which they shed freely abroad in all directions, and that these are borne in infinite numbers through the atmosphere, which they pervade near the surface of the earth in all places. The fact of their universal presence is made manifest by the promptitude with which fungoid growths spring up in all circumstances in which the conditions favor their development. We know that the numbers of spores which all fungi produce are incalculable. The larger fungi give us evidence of this. The spores of a single puff-ball have been estimated to be more numerous than the entire human population of the globe. It is true that to ordinary observation the presence of foreign matters in the atmosphere is not perceptible, except when such foreign matters take the gross form of clouds of smoke or dust; but particles of

smoke or dust, and in general of all inorganic substances, are so heavy that they soon subside; yet when the air is thus left apparently free from all foreign admixture, it is demonstrably full of organic particles so extremely light as not to subside for many hours or even days of perfect rest. The chemist, it is true, is unable to detect them by his tests, delicate as they are; for being organic, and composed in general of but two or three elements—which elements are in great part those of the atmosphere itself—they produce no distinctive reactions under the ordinary processes of analysis. But there is a mode of analysis much more delicate than even that of the chemist. It is that which has been applied incidentally to this question by Professor Tyndall, in his interesting investigation into the chemical effects of light upon vapors. Professor Tyndall discovered that there are many substances of great volatility which, when in the state of vapor, are easily decomposed by light. He found that a perfectly transparent vapor-like steam, when traversed by a luminous beam, is absolutely invisible; while we all know that if we admit a beam of sunlight into a darkened room, through an aperture in the shutter, the path of the beam through the apartment is as distinctly marked as if it were a solid bar. That this visibility of a beam of light in the air is not owing to the power of the aerial particles themselves to reflect light, is demonstrated by him by proofs entirely conclusive. A beam of light from an electric lamp was made in his experiments to pass through a large glass tube closed at both ends by plates of glass, ground on. No light was permitted to escape into the room; and, accordingly, when the tube was exhausted of air altogether, and no light from its interior was reflected to the eye, it was perfectly invisible. But if the air of the room were allowed to re-enter it, it immediately became brilliantly luminous, as in the case of a sunbeam admitted through a window shutter. He showed, however, that a filter of rather closely compacted cotton will shut off entirely, or almost entirely, the organic matters which the air contains; and he showed, finally, that absolute rest for a long period of time will cause these particles completely to subside. He constructed a closed space, cubical in form and several feet in linear dimensions, glazed so as to permit him to pass through it a beam of light, and to observe the path of the beam. This small apartment was made absolutely air-tight and left to itself. On each succeeding day the brilliancy of the trans-

mitted beam grew less and less; and at length, at the end of a week, it could no longer be perceived at all. The apartment was optically empty.

It is not necessary to suppose that all particles of organic matter are living germs of vegetable or animal organisms; but when we see how constantly such organisms spring up wherever the conditions favor germination, it is impossible to doubt that a vast many of them have this character; and that these are the source of those growths of minute cryptogams which thus seem to spring up spontaneously. There is no mode of accounting for such growths, except to suppose that they are actually spontaneous; and accordingly the view has been taken by some physiologists, perhaps I should say many, that the true mode of accounting for the appearance of microscopic forms of life is to suppose that they originate without organic antecedents, or as they expressed it, *de novo*. No question at the present day is more sharply debated than that which relates to the origin of life. There is no subject which has been pursued experimentally with more zeal, more earnest solicitude to reach the truth, and with more singularly discordant results than this. The notion of spontaneous generation, is not, by any means, of modern origin. It has been entertained by naturalists in every age since the dawn of scientific history. But the earlier naturalists, Aristotle and Lucretius, for instance, conceived that organisms of a high order of complexity, such as insects, or fishes, or reptiles, might be directly produced out of the moist earth softened by showers, or out of the slime and mud of rivers; whereas those of our time have long since abandoned any such extravagant notions, and confine themselves to the assertion that life in its spontaneous origin is manifested only under the simplest forms.

Less than three centuries ago the belief that living things may originate without eggs, or germs, or living parents from which to proceed, may be said to have been universal in Europe. Of the truth of this belief there was supposed to be visible evidence in the invariable occurrence of maggots in putrefying flesh. The doctrine was held as matter of faith, and those who first assailed it were naturally accused of impiety and irreverence. Prominent and perhaps first among these was Francis Redi, an Italian philosopher, scholar and poet, born in 1626. He presented a conclusive disproof of the spontaneous generation of maggots in putrefying flesh, by

simply inclosing, in an open mouthed jar covered with gauze, pieces of flesh still sound, and leaving them in the sun to putrefy. Putrefaction occurred as before, but no maggots made their appearance. The maggots, nevertheless, did appear on the gauze, and a little observation made their origin manifest. The flies, of which they are the progeny in the larval state, being attracted by the odor of the flesh, but unable to reach it, laid their eggs upon the covering of the jar, and out of these the larvæ were presently developed. Having demonstrated the falsity of the popular belief on this subject in a case so conspicuous, Redi naturally generalized his conclusion, and took the ground that no living thing comes into existence without deriving its life from something previously living. He did not say, as it has been said later, "*omne vivum ex ovo*," but "*omne vivum ex vivo.*" He still believed that out of a living plant may arise a living animal, as the insect within the gall of the oak, or the worm within the fruit which presents no external puncture. His doctrine was, therefore, that which Huxley has named *biogenesis*, in contradiction to spontaneous generation, called by him *abiogenesis*, and by Bastian *archegenesis*. But *archegenesis* had been put aside only to return again under a new form. Among the earliest revelations of the microscope was the remarkable fact that, whenever a dead organic substance is infused in water, myriads of minute creatures presently make their appearance in the infusion, all possessing most extraordinary and many of them very varied powers of reproduction. They multiply by means of *ova*, by means of buds, or gemmation, and by means of self-division, or fissuration. All this was strongly favorable to the doctrine of *biogenesis*. Where so many means of reproduction existed, every one of them so effectual and sufficient, to provide that the same forms of life should be produced without any organic antecedents, seemed "wasteful and ridiculous excess." This view, however, met here and there with a dissentient. About a century and a quarter ago, John Thurberville Needham, an English naturalist, resorted to an experiment which, with various modifications, has been since repeated many hundreds, possibly many thousands, of times, with the view thoroughly to test the question whether, in its application to infusorial life, the doctrine of *biogenesis* is universally true. He prepared an infusion, thoroughly boiled it in a flask, corked it tight, sealed the cork with mastic, and covered the whole with hot ashes, designing to destroy

by heat any germs which might be in the infusion, in the substance infused, or in the air above the liquid in the flask. After some days or weeks, he found that, notwithstanding all these precautions, living organisms did make their appearance in the flask, precisely such as, in freely exposed infusions, habitually appeared earlier. This experiment was immediately repeated by Spallanzani, an Italian ecclesiastic and naturalist; but Spallanzani, instead of corking his flask and cementing his corks, sealed the vessels by fusing the glass; and having thus completely cut off communication with the outward air, kept them at the boiling temperature for three quarters of an hour. No life appeared in the infusions of Spallanzani, and the doctrine of *biogenesis* was again apparently triumphant.

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HAIR IN ITS MICROSCOPICAL AND MEDICO-LEGAL ASPECTS.

THE examination of the hair in its medico-legal relations is a subject hitherto but little noticed, except superficially. Yet many cases might be mentioned in which the microscopic examination of the hair was of great importance. In the medico-legal examination of hair, two questions are met: *Are the hairs from animals or from men? and in the latter case from whom do they come? From what portion of the body?* Of course, if the hairs belong to a beast, that may be sufficient to settle the question at issue; but the difference between such and human hair has been too little noticed. A human hair under the microscope shows three distinct layers: the outer, cuticula, or the superficial covering, formed of epithelial cells, with rounded contour, lying over each other like tiles, which clothes the surface of the hair from its exit from the skin to its end. The ends of the scale stand out somewhat from the shaft, and give the outer circumference of the hair a more or less jagged appearance. Seen sideways, the cuticula appears as an undulatory design, more

prominent if the hair is treated for a short time with concentrated acid. The scales have their points directed towards the free end of the hair; hence the latter can be easily distinguished from the other broken end.

The cortical substance forms the principal part, and often the whole of the shaft. It consists of a system of closely-packed cells in rows lying nearly parallel to the long axis of the hair, giving the cortical substance an appearance as if striped lengthwise. These cells are so intimately united that without reagents this striped appearance alone shows the cellular structure. Concentrated sulphuric acid breaks up this union, and reveals the spindle-shaped cells, with occasionally a nucleus. The cortical substance has different colour, according to the colour of the hair; generally the colour is diffused through its whole mass; less frequently the colour depends on granular pigment scattered through its substance in small masses. Finally the cortical substance contains a number of cavities filled with air, most evident in the hair from aged persons or in dry hair. These are secondary results of drying, as they are not found in young hair.

The central portion, the medullary substance, forms, when well developed, an axis-cylinder, one-fifth or one-fourth the diameter of the hair, with sharp outlines, generally central, but many times a little eccentric in position. The medullary substance is not constant; it is often wanting in human hair, especially in blond hair. It is wanting less frequently from hair obtained from other parts of the body than in that from the head. In woolly hair it is always wanting; also in the hair of the new-born child. The medullary substance is often interrupted, and sometimes consists only of a few dark points lying in the axis of the hair. The nature of the medullary substance is still a matter of dispute, some considering it cellular, others denying this. The first is certainly the correct view, as may be seen by following the development of the medullary substance from the papilla, where round and imperfectly polygonal cells can be seen gradually merging into the medullary substance. The medullary substance has been thought to contain the pigment; this is not so, the supposed pigment-granules being very minute air-bubbles. The cause of the colour of the hair is found in the diffused pigmentation of the cortical substance. The cause of the hair becoming grey or white, is to be found in the disappearance of the diffuse pigmentation of the cortical substance, the cause of which is not yet

known. The medullary substance can be more easily seen in white hair than in coloured.

Turning now to the hair of animals, we find generally the same three layers as in human hair, but differing to such a degree that, as a rule, a hair can be easily recognized as belonging to an animal. The cuticula in most animals has absolutely and relatively larger cells, which give the hair a characteristic appearance, as is seen especially well in the wool from sheep. A toothed or saw-like appearance of the contour of certain animal hairs depends upon the larger development and peculiar relations of the cuticular cells, whose points stand out so far from the hair that the latter has a feathered appearance, as in a field-mouse. Among animals the greater bulk of the hair is formed by the medullary substance, the cortical substance being only a thin layer; often, indeed, is reduced to a hem-like streak. This predominance of the medullary substance is seen best in the shaft of the hair; towards the end the cortical substance predominates, the medullary becoming thinner. Generally, the cortical substance has the same structure as in human hair, and the same variety of pigmentation; in some animals, as the cat, rat, and mouse, the cortical substance is more translucent and of finer structure, resembling, under the microscope, a hyaline envelop of the medullary substance. The medullary substance in animals is an interesting study, differing greatly from the same layer in human hair. The cellular structure is generally very evident, without the employment of any reagent. The cells vary greatly in size and form.

Though the hair of animals usually is so different from human hair that it can be easily recognised, yet the difference is sometimes less marked; especially may this be the case with single hairs, and at times only a single hair can be had for examination. This resemblance is caused by the absence of the medullary substance. Dogs' hair, especially when brown, is often very similar to human hair, or may be almost exactly the same; fortunately, only separate hairs are thus similar, while generally the remaining hairs which are given for examination have clearly the animal type. Reagents will often help to decide the question.

In medico-legal cases, when it has been decided that the hair examined is human hair, the question arises, from whom it comes, and from what portion of the body. In regard to the first question

it may be merely said here that the hair examined must be compared with that of the person concerned, both in regard to its gross appearances and microscopically. In deciding to what part of the body the hairs belong, the length, the size, the form, and the root of the hair, must be noticed. The hair from the head and beard is less limited in its length than the hair on other portions of the body; though individual and other circumstances may modify the length of the hair from the head and the beard. The size of the hair differs in different parts of the body, and so may form a diagnostic mark. The beard is the thickest generally, measuring 0·14 to 0·15 mm.; next comes the hair about the female genitals, 0·15 mm.; then the eyebrows, 0·12 mm.; the hair about the male genitals, 0·11 mm.; finally, the hair from the head in either sex, 0·06 to 0·08 mm. The other individual differences which are found may render the value of the size for diagnosis less reliable. Moreover, it must not be forgotten that the same hair may vary in diameter. The shape of the hair modifies its diameter; thus cylindrical hair especially is found only on the head; but when this is curly it is flattened, and the transverse section is then oval instead of round. The beard is generally triangular on transverse section, with one convex side; the hair from the genitals is generally oval, sometimes triangular. Hair which has been exposed to the action of the sweat is sometimes swollen in one part, and so changed in form.

When the hair grows undisturbed it ends always in a fine point. All the hair of a new-born child, hair which grows at the age of puberty, and such as has grown naturally without interference, always has a pointed end, which may be of use in deciding in regard to the age of a person. Later this normal ending is not found. Hair which has been cut has at first a sharply-defined transverse section; later the edges are rounded off, and the end becomes round and diminished in size, or is frayed out. This may lead to an approximate calculation of the time which has elapsed since the hair was last cut. The beard, being less frequently cut, is more often split and frayed out. The hair from the female head, generally not cut, ends regularly in two to three points, often in more, each having the end frayed out.

The shape taken by the ends of the hair depends upon the action of friction and sweat, the former splitting and rubbing off the ends, the latter macerating and acting chemically by dissolving or soften-

ing the connective substance. The shaft of the hair is acted upon by the same agents and changed; especially active is the sweat, changing the colour, as is seen in the axilla, on the scrotum, and the labia. From the form of the hair, especially of its end, we can draw conclusions as to the nature of the influence to which it has been exposed, and by means of this and its other peculiarities we may be able in medico-legal cases, with more or less certainty, to decide from what part of the body it came. But no form of hair is absolutely characteristic of any portion of the body.

E. Hofman, M. D.

THE COLLECTION OF LEPIDOPTERA.

INTERESTING as is the study of butterflies and moths in the larval and pupal states, it cannot in any way compete with the interest excited in the entomologist's mind by the *imagines* of the "mealy winged" tribe of insects. The beauty and grace of the "full fledged" butterfly would alone be sufficient to account for this; but when we take into consideration the variety existing in the methods of capture and preservation, it is not difficult to understand how a pursuit which brings into play every bodily and mental faculty should exercise such a fascination over its devotees. In entering upon a description of the different methods of collecting the perfect insect, it seems advisable to take first the butterflies, which form a group very distinct from the moths, whether diurnal or nocturnal. The apparatus required is neither costly nor complicated. The most important item is a net, of which article there are three notable kinds, besides many hybrids. In the first place there is the clap-net, a form which is not much in vogue at the present time, but which nevertheless has many advantages. It is especially useful in a "stern chase" with a strong-pinioned insect. It is made of two sticks of from four to five feet in length, usually constructed in two or three pieces joined by ferrules. The top joint of each stick is bent, and the two are hinged together. A piece of green gauze or

leno is fitted on and stretched between the two sticks, and the net is complete. It is a very cumbrous piece of machinery, and this is, in fact, the great objection to it. The next description of net is the ring net, the most inexpensive and commonest of all. The best way of making it is to get a three-branched Y-shaped socket, which may be had at any dealer's for sixpence. Into the lower socket of the Y a walking-stick is fitted, while the two smaller tubes hold the extremities of a cane ring, on which the net is hung by means of a loose hem. When not in actual use the cane ends can be removed from their sockets and the ring twisted up and carried in the pocket. Another form of this net is constructed of a single piece of thick wire or metal which is jointed in three or more places, so as to fold up easily, and made to screw into a brass tube, the other end of which receives the handle. This species of net is small and rather heavy, but it has the advantage of great strength and is very portable. I have procured a fair-sized one for 4s. 6d. The handle of this net is sometimes constructed on the telescopic principle, with three, four, or six joints, any number of which may be used, as occasion requires, and the whole goes into the pocket. The utility of this ingenious contrivance is doubtful; a walking stick is such a useful article to the entomologist that in most cases he would rather be with it than without it, whether he had a net in his pocket or not, and an umbrella forms no bad substitute for a stick as a net-handle in showery weather. The third and last description of net which I shall attempt to describe is that which goes by the name of the umbrella-net. It is formed of two pieces of jackspring, hinged on to two pieces of brass, the top one fixed and the bottom one movable, as in an ordinary umbrella. When up, it forms a large ring-net; when down, and covered with an ordinary black glazed sheath, it passes very well for an umbrella. The only objection to it is the curious spectacle presented to passers-by during a shower of rain, of an infatuated mortal with a good umbrella which he does not put up. A net of this description may be purchased at prices ranging from five shillings to half a guinea, but it is the truest economy to get a thoroughly good one, cost what it may.

Next in order to nets in a list of necessaries come pocket-boxes. They are made of wood, tin, and zinc. The wooden ones have the neatest appearance, and are cheapest, costing sixpence and upwards; but the zinc have the great advantage of retaining moisture, and thus

preventing the dead captures from getting dry for a long period. The tin boxes have the same good points as those made of zinc, but are liable to rust. A few pill-boxes should also be kept in the pocket continually; as the collector never knows when he may come upon a desirable specimen. Nested willow-chip pill-boxes may be had at 3d. per dozen. I will now say a few words about the different kinds of killing apparatus in use, which, as well as the boxes and nets mentioned above, are of course equally adapted to the exigencies of night work among the moths.

The cyanide bottle is constructed as follows: Take a wide-mouthed glass bottle, such as may be procured at any chemists for a few pence, and place at the bottom a layer of cyanide of potassium; then mix plaster of Paris with water till a thick cream-like mixture is produced, and lay this over the cyanide; when the compound is set, a few thicknesses of blotting paper may be spread over it, and the bottle corked with an air tight bung or wooden stopper. It ought to be mentioned that the cyanide of potassium is a virulent poison. This killing bottle may be had with a metal band round the body, to which is fastened a ferrule for the reception of a stick. The rim of the mouth is cased in gutta percha, to soften the effects of any collision, and the whole contrivance is designed to facilitate the capture of insects on high walls, lamps, and other situations inaccessible by ordinary means. The *modus operandi* is very simple; the bottle is hoisted on the stick, the mouth being slipped over the desired insect, which is quickly stupefied and slain *in statu quo*. A chloroform bottle is often useful. It is constructed of brass, the liquid flowing through a minute perforation at the top, over which screws a metal cap. Only a drop runs out at a time, but in a closed box this is quite enough to kill all lepidoptera contained. An oval tin box with a lid at each end and a perforated division in the middle makes a good killing implement, but scarcely strong enough for any work requiring rapidity of execution. It is charged by placing well-bruised laurel leaves in one division, the fumes of which ascend through the perforated division, and after a short interval prove fatal to any moths placed in the other compartment. A few dozen entomological pins must be carried into the field, either stuck in the cork of the pocket-box or in a pin-cushion suspended inside the coat. These pins are made in all sizes, and may be procured, either gilt or plain, from any dealer.

The above-mentioned articles constitute the whole *paraphernalia* necessary to the butterfly hunter. It only remains to make a few observations on the haunts of butterflies, and the manipulation of the net. Many species are very local in their distribution, being confined to one or two fields, or, it may be, a small copse. It therefore behooves the collector to leave no portion of a district unexplored; and for the same reason he ought to catch every insect on the wing whose species he cannot certainly declare. In this way many rare Fritillaries and Blues have been captured, which would certainly otherwise have gone free. Some butterflies have favorite flowers on which they especially love to sit. The Red Admiral and Peacock have a predilection for the thistle, the Silver-washed Fritillary for the bramble, and the glorious Purple Emperor for the loftiest branches of the oak. To catch this last insect a net mounted on a pole 30 feet high is necessary, unless his Majesty can be induced to descend from his lofty position. As he is very fond of dead animals, tainted meat, and other substances of the same nature, the collector should keep his eyes open when near any of these in a wood haunted by Emperors. In using the net it is often better in the case of the swifter butterflies, to pop it over them suddenly, taking them by surprise. If by any chance they become alarmed, and take themselves off, it is a race between human legs and lepidopterous wings. If there are no hedges or other obstructions in the way, the former have usually the best of it, and the butterfly finds itself in the net, which should be constructed of white or green leno. This material costs fivepence or sixpence a yard, and is consequently much cheaper than grenadine, a silk fabric which is said by some to be very superior. Green dyes are apt to rot textures which are dipped in them, and for this reason a white net is preferable; but on the other hand it has the demerit of being very conspicuous. Instead of placing them in a killing-bottle, butterflies may be slain by puncturing them with the first finger and thumb, just beneath the thorax. Personally, I prefer the cyanide bottle, as the pinching is apt to spoil the specimens. The methods of setting and preserving butterflies are the same as those applied to moths, and I shall therefore reserve all hints on these heads for a future letter. In my next I hope to describe that artificial mode of attracting nocturnal moths known as "sugaring."

M. E. S.

THE SILICEOUS SHELLED BACILLAREÆ OR DIATOMACEÆ.

(*Continued from page* 137.)

Now, at the conclusion of these short historical outlines, I will pass on to my own labours. The already mentioned treatise of Leiblein in the *Regensburg Flora*, for the year 1830, gave me the first impulse for the investigation of these little organisms. I examined the diatoms of the neighborhood of Schlensingen, and found, not only most of the forms described by Leiblein, but also others, not yet described. On this occasion I must thankfully acknowledge how kindly Prof. Leiblein answered my first questions for instruction, and how very much I was aided in my earlier studies, by the use of his collection of algæ, gathered near Würzburg, among which were also diatoms. But I am also not less obliged to the Herr Pastor Frölech in Boren, near Schleswig, and Von Martens in Stuttgart, who furnished me, most kindly, plentifully with material from their collections. In the following years I continued my investigations of these microscopic forms, just as diligently as I had begun them, and in the year 1833, while I lived at the University of Halle, I was enabled to publish, that year, seven decades of my "*Algæ aquæ dulcis germanicæ*," with dried specimens, among which several diatoms were also given. In the same year I published in the *Linnæa* the *Synopsis Diatomearum*, of which I had special impressions struck off, that I gave in commission to Schwetschke in Halle. These bear erroneously the date 1834. In this pamphlet I, for the first time, separated the true Diatomaceæ, with shell (schale) already designated as hard and glassy, (p. 3) from the softer shelled forms, which I called *Desmidieæ*. This work has been estimated very differently. Meyen (Weigm. Archiv., 1835, § 210) complains that everywhere a too great desire for new species was manifested, and yet, it afterwards appeared, that not only all the species established by myself stood proof, but even many a form, mentioned by me as a variety, was established by others as distinct species. Ehrenberg took the trouble in his third "*Aid to the knowledge of larger organisms, in the direction of the smallest space*," to reduce most of the forms established by me in the Synopsis, to such forms as were known to him; but

later, he has established the same forms as distinct species in the larger Infusoria-work, although often with suppression of the names given by me, designating these as synonyms of forms known before, and where they did not belong.* The proof of this will be given at the respective places. I had however without knowing of Ehrenberg's labors, already in the first pages of my Synopsis correctly represented the structure of the frustules (Panzers) as two plates, and had also mentioned the frequent striæ in many forms. The openings of the *Frustulieæ*, (Naviculeæ) were then indeed as unknown to me as to Ehrenberg, who mentions them later. The want of a good microscope at that time, prevented me from carrying out my investigations with the necessary promptness. Only a short time before the printing of my *Synopsis*, I had the opportunity, through Herr Von Schlechtendal, to use a Schiek's instrument, and by its help to make some improvements in my drawings. These are figures 12, 31, 21, 22, 23, 32, 33, 35, 41, 43, 45, 53, 54, 55, 57, 60, 61, 62, 63, 64, 65, 66, which, however much they have been censured, still give a faithful representation of the objects, and are better than the then existing representations of Bory St. Vincent, Turpin, Lyngbye, and even of Nitzsch ; nor do Ehrenberg's figures, made at the same time, in his large Infusoria work, surpass them. That the remaining figures are, in fact, very insufficient, I myself confess, but I content myself the more easily, since I can correct that mistake here, and as I know that Ehrenberg did not fare better than myself with his first representations. If one looks, for instance, at Ehrenberg's figures of the *Echinella Splendida*, (Taf. 19, II,) of the *Gomphonema discolor*, and *rotundum*, (Taf. XVIII, VII, VIII,) of the *Bacillaria Cleopatræ*, *Seriata flocculosa*, and *Ptolemæi*, (Taf. XV, III, VIII, IX, X,) in the large Infusoria work of 1838, it will be confessed that it is quite as difficult to decipher these forms, as those mentioned in my Synopsis. It is also true, that Ehrenberg has mentioned one and the same object several times, and under different names; this is certainly the case with *Fragilaria rhabdosoma multipunctata*, *bipunctata*, *angusta*, *scalaris*, and *diophthalma;* if the drawings are correct, all belong to one and the same species.

*This conduct of Ehrenberg has already been censured by others. Thus speaks Ralfs (" On the British Species of Gomphonema," in the Annals and Magazine of Natural History, Vol. XII, Dec., 1843, p. 462.): " It is greatly to be regretted that Ehrenberg has in so many instances disregarded the names previously affixed by Agardh and Kützing. To alter a name once bestowed is not only discourteous to the first describer, but creates confusion and tends to encumber the science with synonyms; for if it be allowable for one writer to alter a name because he fancies that a new one is more appropriate, succeeding writers have an equal right to alter his names, and in the absence of a recognized rule, some naturalists may prefer one name, and some another."

However insufficient the microscope was with which I at that time undertook my investigation, still I have, by it, made my most excellent discovery, viz., the siliceous valves of the diatoms, which soon led, through my friend, Henri Fischer, in Pirkenhammer, near Carlsbad, to the other important discovery of the fossil deposits of these organisms. I had already, in my "*Synopsis Diatomearum*," called the substance of which the forms of the diatoms are made "glassy," because I had, indeed, even then, suspected siliceous earth in these frustules (*panzers*). I communicated this supposition to my friend, the apothecary, Bilz, an expert, equally renowned as botanist and chemist, at the same time asking him whether he would investigate, chemically, specimens which I would send to him. Bilz answered that my supposition might be correct, but declined the commission, stating that he had no practice in the chemical investigation of microscopic objects. For a short time, I let the matter rest, until again I was reminded of the probable siliceous frustules of the diatoms, on the occasion of investigation of some *Characeæ;* this was the day before Ascension Day, May 17, 1834. I had placed some *Chara* in very dilute muriatic acid, in order to remove the lime-crust, that was in the way of microscopic investigation. In the course of the examination I found that the soft Chara stems were on the outside garnished all over with diatoms, which were not at all affected by the acids. Notwithstanding the twilight, that had already commenced, I treated these diatoms, in separate watch-glasses, with concentrated acids, applying muriatic, nitric, phosphoric, and fuming sulphuric acids. The color of the internal parts became, under the first influence of the acids, beautifully green; but the further investigations with the microscope had to be postponed to the following day. After a sleepless night, the examinations were continued, at the break of day on the 8th of May, and at 8 o'clock A. M. of the same day I had not only the full certainty of the siliceous character, but also of the iron contents of the diatoms. The results of the investigation I give here in the words written at the time, because they have not been hitherto printed:

"The diatoms which had been brought into contact with the concentrated acid (they consisted of *Synedra splendens*, *Cymbella gastroides* and *maculata*) had not changed, otherwise than that their internal matter had disappeared. Now, as I had preserved

dried supplies of other species, too, the investigations were further continued with these.

"1. Experiment with *Melosira varians*. 10 grains of material, dried in the air, were heated in a platinum crucible, over a spirit flame. The gray-green color of the *Melosira*, browned, and became black, as the mass smoked, and emitted an animal smell similar to burnt hair, cartilage, etc. Continuing the glowing heat, the organic remains were totally removed, and there remained a residuum, in which could still be recognized, quite as distinctly as before, the whole portion of the Melosira threads, only they were more discolored, and had assumed a grayish white appearance; but the burning of the organic parts was over very quickly; the rest weighed still 9½ grains, the weight of the organic parts that had disappeared in the burning was therefore, proportionally, very insignificant. Under the microscope, the Melosira members which had been left appeared unchanged, save only that their internal contents had disappeared. They quite resembled those which had been treated with strong acids, only the coherence of the threads which are formed by the multitudes of individuals put together side by side, had become weakened; for whereas, the unheated threads could be boiled in water without losing their coherence, the heated threads were separated into mere single members by the boiling with water. The same happened also to the unheated threads when boiled in water to which had been added considerable muriatic or sulphuric acid.

"A small portion of the heated individuals was next poured into soda in a small platinum spoon before the blow-pipe, but held in such position that neither the reduction or oxidization flame of the blow-pipe came into contact with the fusing mass. The solution of the mass in the soda followed completely, and with effervescence, and I obtained a perfectly transparent glass, in which, however, after cooling, a vitriol green color indicated the presence of oxide of iron. Hereby the siliceous earth was unmistakably proved to be the main part of the diatom frustules. In order to separate it in a pure state, a quantity was poured into a large proportion of soda, dissolving this mass in water, I obtained a siliceous solution, from which I separated the silex, transparent, and in a jelly-like hydrate state, by means of sulphuric acid.

"2. Experiment with *Achnanthes salina, Synedra ulna, Synedra subtilis,* and *Navicula thuringica*. I had collected these diatoms in

1833, in the salines at Artem in Thuringen, and dried them in pretty large quantities. These forms, under influence of the acids, behaved quite like the *Melosira* in the preceding experiment. Treating them with soda, in the platinum spoon, before the blowpipe, with the same care as mentioned above, under No. 1, I obtained a glass, which while hot was colored brown, and after cooling, intensely yellow. The same was the case when borax was used instead of soda, the latter, in the reducing flame, was dark brown during the heating, but after cooling, bottle green. The reaction of iron showed itself here, upon the whole, stronger than with *Melosira varians*, the experiments moreover prove that the iron existed here as a sub-oxyd, and there as an oxyd.

"In order to find out whether the iron was to be looked for in the frustule itself, or in the internal matter of the individual, I repeatedly boiled a quantity with muriatic acid, and then diluted the acid with fresh water. The first boiling indicated a very strong iron reaction, when heated with ferro-cyanide of potassium, for a considerable quantity of prussian blue was obtained. It is, in general, very easy to convince one's self of the presence of iron in the diatomaceæ in the following manner: Some distilled water having been acidified by hydrochloric acid, a few drops of a solution of yellow prussiate of potash is to be added, now, if only a minimum of a diatom is put into this fluid, a girdle of prussian blue will instantly be formed round about it, especially if the iron, as in the latter case, is present as an oxyd.

"The residue of the siliceous frustules, left after treatment with acid, and which before that treatment had a brownish red appearance, had changed color to a gray green, which became a little lighter when dried. During the heating in the platinum spoon, whereby the same smell of burning animal matter was produced, I also remarked, that a cork wetted with hydrochloric acid, caused over the fuming mass, stronger and thicker fumes, which no doubt came from the formation of sal ammoniac.* The ignited residue, was, after the total burning of the organic matter, punky white, and gave also with soda, before the blow-pipe, a tolerably white glass."

* Of the formation of ammoniacal gas during the heating of these bodies, one can be easily convinced when the operation is conducted in a test tube, and a wetted curcuma paper is held at the opening, it will become brown by the escaping ammoniacal gas.

From these experiments, it follows:

1. "That the soft internal substance of the diatoms contains nitrogen (from which I at first drew conclusions that they were more of an animal than a vegetable nature).

2. "That the frustule (*panzer*) consists of pure silica.

3. "That the internal part, besides the soft organic constituents, also contains iron in considerable quantity.

4. "That the color belongs only to the internal parts, and is partly dependent upon the amount of iron present, but that the frustule itself is colorless."

These investigations were sent by me to Herr Alex. von Humboldt, for communication to the Royal Academy of Science at Berlin, which commissioned Messrs. Rose and Ehrenberg to examine my statements. It is known that both these scholars confirmed them, but it is perhaps not known, that I expressed in vain the wish to have the results of my investigations printed in Poggendorf's Annals of Physics and Chemistry; all I obtained was, Ehrenberg furnished a poor and short report of my researches, in which only the siliceous frustules were mentioned, but nothing was said anywhere of the simultaneous discovery of iron in the internal parts; therefore, I was somewhat astonished to see in Ehrenberg's large work on Infusoriæ, p. 244, the iron of the *Gallionellæ* mentioned as his discovery, while he does not even allude to the fact that I, in my essay sent to the Berlin Academy, mentioned iron as a *general* constituent of the diatoms. It is easy to believe here in a "*Turpinate.*" I myself, however, am morally convinced that in reading my paper at that time, Ehrenberg was much too busy with the siliceous frustules to pay attention to the other theory mentioned in my communication, and therefore, that when, several years later, he found the iron himself, whereto he was led through his so-called *Gallionella feruginnea*, he did not remember that I had proved the same in 1834. This collision would at any rate have been avoided, if Poggendorf had published my short article in the Annals of Physics and Chemistry, for which it was quite adapted.

In the following year I began my journey to Delmatia, Italy, and to Switzerland, on which occasion I became familiar with the diatoms of the Adriatic and Mediterranean Seas. The collections made on this occasion were large; but more important than their forms was the observations of the organs of fructification in the *Schizonematic* forms, which are quite analogous to those of the algæ, and find no analogue among animals. An excellent microscope by Schieck, of

Berlin, enabled me to make accurate observations.* At the same time I visited Carlsbad on my return, where already, in 1827, C. Agardh had made a rich collection of diatomaceæ. This locality was of the greatest importance for my researches, since, only a few months before, I had examined the diatoms of the hot baths in Abano, and Battaglia, in upper Italy, as well as those of Tenk, in upper Wallis, as to the lower organisms. At the same time, I have thankfully to acknowledge that during my presence at the above mentioned baths in upper Italy, I was most liberally aided by my friend, Dr. Biasoletto, in Trieste, who accompanied me there, and Herr Professor Meneghini, at Padua, who received both of us in a very friendly manner, and gave to me several algæ from the Euganean baths. I cannot less praise the friendly reception and assistance I received from Herr Fischer, in Carlsbad, the same who afterwards discovered the existence of diatomaceæ as fossil at Franzensbad. In the year 1839, I made a second journey to the sea-coast, and remained a few weeks on the Oldenburg coast, and the islands Wangerorge, Helgoland, and near Cuxhaven. This journey too was very favorable to my studies, because it procured for me, in addition to the treasures of the sea, the acquaintance of a man, who from his excellent collection of algæ, which, from its fullness, may be called one of the first in Germany, with extreme kindness not only gave to me many rare algæ, of which mention has already been made in my *Phycologia Generalis*, but left to me also for this work, the use of his whole rich collection of diatomaceæ; I speak of Herr Senator Dr. Binder, of Hamburg. With not less thankful feeling I acknowledge the kindness of Herr Apotheker Souder, in Hamburg, whereby he, in sending to me the sediment of the sea from the mouth of the Elbe, near Cuxhaven, gave me the opportunity to become acquainted with the interesting diatoms of the chalk described by Ehrenberg, which was the more agreeable to me inasmuch as a request I had made to Herr Ehrenberg (to whom I had formerly imparted dried specimens of the forms described in my *Synopsis Diatomearum*, and had also offered the diatoms collected from the Adriatic and Mediterranean,) had not been noticed. In my present undertaking I have also been aided by Herr Fischer, of Pirkenhammer; Dr. Philippi, of Cassel; Prof. Phöbus and Dr. Gumprecht, by communication of fossil forms; also, from Dr. Montague, of Paris, I have received several rare forms described by

* These observations were wrongly interpreted by Kützing.

him, from the open sea and the Antilles sea; also, Herr Berkeley, of Kings Cliff, England, sent me many rare forms described by Ralfs. Some New Holland forms were found in an earth lump mixed with algæ, which Herr Dr. Preiss, of Herzberg, had brought with him attached to a Nautilus shell; other forms I found on algæ in my collection, from the Indian Ocean, from the Cape of Good Hope, from Corea, Japan, Kamtschatka, Peru, Chili, Jamaica, and the Canary Islands. The beautiful *Terpsinoë Musica*, was, with several other forms, found in the rude hairs of a Marchantia from tropical America, in the collection of Herr Senator Binder; it had been discovered several years previously by Herr Lindenberg, and by him given to Herr Binder, who had referred it to the diatoms. At the same time I also found different, and in part, peculiar forms, in the various collections of algæ sent to me for naming by Burgomaster Jürgens, and Dr. Roch, in Jener, Chief Assessor Römer, in Clausthal, Dr. Rabenhorst, in Dresden, and Major M. Flotow, of Hirschberg. Now, when already, at the end of March, 1844, twenty-nine plates were printed of this work, wherewith I had expected to close it, and also the greater part of the manuscript already finished, I received, through Herr Apotheker Souder, of Hamburg, a collection of diatoms which Herr Apotheker Kruger had collected in the fresh waters of Trinidad, and at the same time the Herring's collection of diatoms, which Herr Senator Binder had purchased, were sent to me by this gentleman. Such unsolicited, rare kindness rejoiced me the more when I found that the latter collection was rich in those forms which Herr de Brébisson had described. All the species were collected by Herr Lenormand, and prepared for the collection with a neatness peculiar to him. The multitude of new and authentic specimens which came before me by these communications induced me to engrave the plate xxx, in addition, and when I had already finished a great part of the admitted figures, a considerable package of dried diatomaceæ came from Herr Meneghini, of Padua, which also furnished some new additional forms, part of which could yet be embodied in the plate xxx. Lastly, I received, during the printing of the manuscript, from Dr. Dickie, of Aberdeen, a very peculiar sea form, from the coast of Scotland, of the Navicula group; it is described as *Dickiea ulvacea*.

(*To be continued.*)

Prof. H. L. Smith.

ON THE UTILITY OF $\frac{1}{50}$TH *OBJECTIVES.*

I BELIEVE that microscopists have quite generally doubted the usefulness of extremely high powers. In fact, a few months ago the published work of the best $\frac{1}{50}$th objectives then made was decidedly inferior to that of $\frac{1}{10}$ths to $\frac{1}{20}$ths by our best makers. Dr. Carpenter, in speaking of high powers, obtained either by the use of deep eye-pieces, or a $\frac{1}{50}$th inch objective, says: "It is questionable whether anything is really gained thereby." Dr. Beale, however, speaks favorably of his $\frac{1}{50}$th made in 1864. Dr. Woodward "regards immersion $\frac{1}{8}$ths and $\frac{1}{16}$ths of Powell and Lealand as superior in defining power to the dry $\frac{1}{25}$ths and $\frac{1}{50}$ths of the same makers." Latterly, Powell and Lealand have constructed at least one $\frac{1}{50}$th on the immersion principle, but neither their $\frac{1}{50}$th nor their new $\frac{1}{80}$th, as far as is known to the public, has shown any decided advance. No work by them superior, or perhaps equal, to the performance of a good $\frac{1}{10}$th to $\frac{1}{20}$th of any of the leading makers has been thus far reported.

On the 12th of March, 1873, Mr. R. B. Tolles filled my order for a $\frac{1}{50}$th immersion, of 150° angle of aperture or upward. As invoiced by Stodder the angle is 155°, although Tolles got more. Of the construction of this exquisite glass I know nothing, but of its performance I can now speak with confidence. It is believed that the success of this lens has demonstrated the utility, if not superiority, of very high powers on the classes of work to which only they are adapted.

In determining the resolving and defining power of the objective, the ordinary test diatoms were used. The results, both by monochromatic sunlight, (with *ammon. cup. sulph.* cell,) and the light of a small lamp, are given in the following table:

TEST.	LAMP.	MONOCHROMATIC SUNLIGHT.		
	Transverse Lines.	Transverse Lines.	Longitudinal Lines.	Dots.
Amphipleura pellucida.....	Distinct.	Easy.	Distinct.	Distinct.
Frustulia Saxonica	Easy.	"	Easy.	"
Surirella gemma.....	"	"	"	Easy.
Pleurosigma fasciola.....	"	"	"	"
Navicula crassinervis.....	Not tried.	"	Satisfactory.	Distinct.
Grammatophora subtilissima..	Easy.	"	"	"
Striatella unipunctata.....	Not tried.	"	Distinct.	"

With lamp it was easy to show the longitudinal lines and the dots on *Surirella gemma* and *Pleurosigma fasciola*. The above tests

were mounted dry, with the exception of *Grammatophora subtilissima*, which was in balsam. By lamp light the work is, of course, not as good as with the blue light, but I do not notice any greater difference than is made by other objectives.

I possess a good $\frac{1}{15}$th immersion, (really about $\frac{1}{18}$th,) by one of the most distinguished makers. This lens has kept fully up to the latest published work of the "highest authorities." The superiority of the $\frac{1}{60}$th is at once manifest with any illumination, and on any difficult test. I instituted a direct comparison on *Pleurosigma fasciola* with lamp, simply changing the objectives and not altering the illumination. Both were used under the same power, viz: 2500 times. With the $\frac{1}{60}$th the object, both sets of lines, and the beads were clearly and splendidly displayed, with plenty of light and to spare; making the strongest contrast with the want of light, general obscurity and comparative poverty of performance of the $\frac{1}{15}$th. There is no danger of making the contrast too strong in this report; no room for exaggeration.

The $\frac{1}{60}$th has also been tested on the *Podura* and *Lepisma* scales with pronounced excellence of definition. Many butterfly scales have been tried, and readily and clearly resolved into the so-called "beading."

I have entered somewhat into detail in describing these results, so that others may be better able to compare the work of this lens with their own. A glass of such high power must necessarily be unsuitable for a delineation of the larger objects, and only well adapted to a study of the smaller living organisms, and the minute details of structure of larger objects generally. In this almost unexplored field of research, very high amplification, combined with the very highest order of defining power is required. In its definition of the edges of Monads, Bacteria, the smallest vegetal germs, their hyaline envelopes, and in tracing their internal changes, the $\frac{1}{60}$th surpasses my expectations. When extraordinary care is used in manipulation, its superiority is as unmistakable in this kind of work; with ordinary daylight illumination, as it is with blue light, in the resolution of difficult diatoms.

G. W. Morehouse.

Wayland, N. Y.

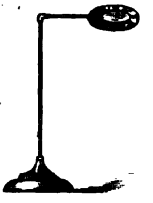
ON THE PREPARATION OF DIATOMACEÆ.

THE following paper is intended as a supplement to the very excellent article by Christopher Johnston, M. D., in a former number of this journal, under the above heading, and I know of no better guide for the student. What I have to say relates to the rapid preparation, from crude material, where this has been at all carefully gathered, and to a mode of mounting, *invariably on the cover of the slide*, not mentioned by Dr. Johnston, but which has some great advantages. The gatherings should not be dried, but kept moist, in phials with a little creosote to prevent mould. I very much prefer to examine whole frustules, with both valves adherent, or if filamentous, still cohering. And I have many bottles of preparations for mounting which are nearly as clean as though they had been treated with acids. And many of the most interesting preparations which I have were never boiled in acids. Of course, very much depends upon the skill and carefulness of the gatherer, and a little patience and judgment will enable any one to obtain the crude material tolerably pure. Only a few days ago I made a gathering of *Nitzschiæ*, in which I have the frustules almost as free from foreign matter as though they had passed through the most elaborate acid and chlorate of potassa treatment.

Supposing, then, that one has before him a phial which will hold a considerable quantity of water compared with the sediment in it, the latter composed more or less of diatoms. We proceed thus, and if it has stood for some days perfectly undisturbed so much the better. The bottle is twirled rapidly, and the lighter material rising up in the axis will soon diffuse itself throughout the water. Allowing it to settle for two or three seconds, until to the eye the grosser portions have just been deposited, all that remains floating is now poured off into another phial, and it is from this stock that we are to separate the diatoms and sand from the clay and organic matter. The material poured into this second bottle is allowed to settle until the water simply appears milky or cloudy; the time will vary according to the minuteness of the diatoms, and can only be judged of from experience, say one minute, when all that remains floating must be poured off, and thrown away, unless there are very minute forms which it may be desirable to separate. The phial is again to be

filled with rain, or distilled, water, (hard or lime water should be strictly eschewed,) and again shaken up. As soon as the heaviest deposit touches bottom, the rest should be poured off into a third phial, leaving say about one-fourth the amount behind in the second phial. This third phial will now consist mainly of sand and diatoms, with lighter organic matter and pure clay; the last two can be removed by elutriation ; for this purpose, fill the phial No. 3 with water, and after well shaking allow it to settle two to five minutes, pour off and throw away the slightly milky water, and repeat the operation, allowing it to settle a somewhat longer time ; the operation may be repeated a third time, when particles, suspended after an interval of eight or ten minutes, may be poured of. Often, after the first settling of bottle No. 2, the diatoms will rise more pure in the mass by twirling the bottle than by shaking it up. A little practice and care will enable any one to separate certain diatoms, according to size. I had a gathering of *Pleusosigma Spencerii* from Scioto river, O., sent to me, but although it had been chlorated, still when a mounting was made, not more than one or two frustules would be in the field of view, the great mass being either smaller forms, or fine fragments of silex; by careful watching and testing the time when the different sizes would remain suspended, I have made from this a preparation, which will show hundreds where before were scarcely any, and which would never be recognized as the same gathering. Supposing now a trial shows us the diatoms tolerably abundant, the trial being made by heating in the manner presently to be described ; the phial is filled with alcohol and water, half and half. Some samples of alcohol leave behind a scum after evaporation, especially noticeable after burning in the mode presently to be described, and water which will leave crystals, or any scum, must be avoided ; the beauty of the preparation will largely depend upon being particular in this matter.

For mounting diatoms I invariably place a drop of the fluid containing them upon the cover, *never on the slide.* The alcohol and water will spread out on the slide, but will remain heaped up on the round cover, like a plane convex lens. I prepare a little stand, represented in the accompanying wood cut, of quite fine wire (so as not to conduct off too much heat) bent at right angles and inserted into a base ; the free end is bent into a ring, and upon this ring is placed a square plate of very thin iron, (such as is used



for the so-called "tintypes" in photography, with the Japan burned off,) held in place by bending the corners of the square over the ring, loosely, to allow expansion, without bending when heated; upon this plate the cleaned cover is placed, and then by means of a pipette, a drop of the alcoholic liquid with the diatoms is placed upon it, and the spirit lamp applied below. The alcohol takes fire and is allowed to burn out; the flame of the lamp is then placed beneath, and the rest gently boiled, the remaining alcohol escaping during this ebullition causes the diatoms, by this very act, to distribute themselves very evenly over the cover, and all matting is effectually prevented. It is better after one perceives that this even distribution has taken place, not to push the heat so as to make large bubbles again, but to slowly evavorate until dry, after which the full power of the flame must be applied until the iron plate and the glass cover is red hot; at first the mass of diatoms, &c., will become black, but as the organic contents and debris burn away there will finally remain only the silex nearly white. I invariably burn in this manner on the cover; even the specimens which have been prepared with acids, for the diatoms thus treated when mounted appear much sharper and cleaner. The amount of heat, if the diatoms are rigidly siliceous, as most of them are, may be the full power of an ordinary alcohol flame continued for some time, but if they are imperfectly siliceous, care must be exercised in the burning.

I invariably use old balsam for mounting, just as bought from the shops, especially if I wish to have a specimen which will bear immediate handling, or be ready to be sent off soon as mounted. Allowing then the cover to cool, while the slide is being cleaned to receive it, I place a drop of the balsam, which must not be fluid, only viscous, on the middle of the slide, and now *with this* pick up the cover from the little stand where it has been heated. The diatoms will be so fastened by the heating, that but few will flow out from under the cover, if any, in the subsequent treatment. I now hold the slide over the flame of the lamp (which should be much smaller than when used for the burning,) until not only all under the cover is a mass of small bubbles, but until very large bubbles, balsam steam, appear; the flame is removed soon as the bubbles are observed all running to one edge. I press down the cover at this place by a mounted pin,

and start them in the opposite direction. This may seem unnecessary, but long experience shows that this is the better way to get rid of them; during this the slide is held somewhat obliquely, the cover is kept from slipping by the pin, and if all the bubbles do not disappear, then with a very small flame heat is applied just beneath the obstinate ones, the slide being held slanting, and that part upwards where the bubbles are nearest the edge of the cover. The description is longer than the actual process, and the slide when cool is ready for immediate use. Perhaps I am wedded to old ways, but after trial of fluid balsams, without heat, I have always come back to the old way; still, for selected diatoms, some of these preparations of balsam are good. If the diatoms are to be mounted dry, always the best way, if for real study, I make a ring of the zinc white in balsam, (sold by the opticians,) and which in a moment or two is sufficiently hard to receive the cover, *and never runs in;* after standing an hour or two I give a finishing ring of same, or the usual black varnish on the outside.

I think any one who will adopt the mode of mounting on the cover, and subsequent heating, as above described, whatever may be the rest of the procedure, will never consent to give up this part, since it effects so even a distribution, and such destruction of residual organic matter, and gives such increased brilliancy to the preparations; sometimes, if the acid has not been thoroughly washed out of acid treated specimens, snappy explosions will occur when the alcoholic mixture is heated; of course, the remedy is to pour off, and replace with pure water and alcohol.

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Hobart College, Geneva, N. Y.

THE BEST TESTS FOR OBJECTIVES.

In submitting this paper for your consideration, I pray that so much of the verbiage as *primâ facie* may appear to be egotistical or presumptive, may be treated with kindness, and not allowed to prejudice your minds until the whole paper has been read, discussed, and calmly considered. A double apprenticeship to the study and practice of the subject enables me to speak in terms so confident and positive that I fear to give offense, even in the initiatory title, viz.: —"The best, the most simple, and unerring tests for objectives." In speaking of definition, in most instances I have adopted square measure, but where practicable I have expressed my words in lineal measure. To view distinctly the five-thousand millionth of an inch is good definition. To view the same space with equal distinctness all over the field is flatness of field. To view an object, and to find it presenting an abnormal state, is distortion. I now propose to treat the Definition and Flatness of Field together, and to submit that there is no test so certain as a series of engravings on glass. For my purpose I engrave a series of plates with letters measuring from one two-hundred-thousandth of an inch to one two-hundred-millionth of an inch. Each engraving is of the Lord's Prayer, varying only in size, commencing about the thousandth of an inch, which is at the rate of over a quarter of a million letters to the inch, and progressively decreasing the size, the next of the series being at the rate of a million letters to the inch, the next two millions, the next three, and the next four million letters to the inch. Having reached this point, and finding the Old and New Testament together consist of three million five hundred and sixty-six thousand four hundred and eighty letters (for the convenience of a standpoint), I say the lastly enumerated test is at the rate of one Bible to the inch, and then engrave the next at the rate of another Bible to the inch, and go on decreasing at the rate of a Bible to the inch, down to fifteen Bibles, or, at the rate of fifty-three million four hundred and ninety-seven thousand two hundred letters to the inch; but when it is remembered that the letters are written within two parallel lines, with spaces above and below for long letters, and to enable one line to be distinguishable from another, I most respectfully submit that, such letters as "a," "e," "o," and "u," although averaged with all other letters, with the capitals, and

including spaces, at the fifty-three million four hundred and ninety-seven thousand two-hundredth of an inch, being actually written within the lines, after allowing for the extra space occupied by capitals, the spaces between words, and the space between one line of writing and the next line, it may be taken that the "e" actually occupies only one-fourth of the average, or, the two hundred and thirteen million nine hundred and eighty-eight thousand eight hundredth of an inch.

The measurement does not stop at this point, as there are other steps to be traversed—one, as to the dot to an "i," I say nothing now. As to the "e," it is self-evident that it is not a spot of black of the previously estimated less than two hundred millionth of an inch, but composed of a bent and twisted line across, and about the two-hundred-millionth of an inch; therefore, the thickness of the line has to be considered, and, taking that at a lineal fifth of the space, the two hundred and odd millionth would have to be multiplied by twenty-five as the square of five, which would bring the square of the line down to the five thousand three hundred and forty-nine million seven hundred and twenty thousandth of an inch —and do not stop there, for that five thousand millionth is itself loaded in, and consists of abraded black atoms, grated in by the cutting edge of the glass letter, which atoms can be seen in different aggregations where the line has not been perfectly filled in, and if at the rate of two atoms of black in the square of the line, the five thousand millionth becomes the ten thousand millionth; if at the rate of twenty atoms of black, the size of the atom is the one hundred thousand millionth of an inch.

I now come to the most important and, to my mind, the most interesting part of the subject, which deals with the tests unblackened. For this purpose I must go back to the square of the line forming the letter, as the five thousand three hundred and forty-nine million seven hundred and twenty thousandth of an inch, that reduced to its square root, gives seventy-three thousand + of an inch linear as the breadth of the line. I mount the same series of slides in the way that M. Nobert mounts his justly celebrated tests—without black—and thus open up a wonderful means of study of the whole subject, helping to afford the power of determining at what breadth unblackened lines become invisible, even when aided by the microscopes of the present day. In this instance the seventy-three thousandth is an absolute line, unbroken by a next line.

When viewing the black lines, ordinary direct illumination is sufficient, but when examining the unblackened lines it becomes necessary to adopt in its turn every available means of illumination, because the cut, being wedge-shaped, each side of the cut, from every part to its very apex, both refracts and reflects again and again the light from the other. Again, the original upper and lower surfaces of the glass refract and reflect the light backwards and forwards; again, the top light flows into the cut, helping to produce the climax which blazes away the cut, as the light of the sun overpowers or destroys the light of a candle. By testing by blackened and by plain unblackened letters, it will be found at what point the power of certain objectives ceases to be effective with transparent objects. I can define the smallest Lord's Prayer when blackened, that is, I can define a line of the seventy-three thousandth of an inch, but have never been able to define the same test unblackened. More than that, although I know the exact spot that it occupies, and mark the spot with an India ink ring before it leaves the machine in which it is engraved, I have never (perhaps because of irritable temperament) been able to discover not merely the line, but the aggregation of lines forming the two hundred and twenty-seven letters of the very small tests, although they become perfectly distinct when black.

It is not necessary to possess more than a short selection of my tests to include general purposes, and in some particular cases a single test will be sufficient.

I now pass to the remaining part of my subject, viz., Distortion, which I believe is not so well understood, simple as it is. For this test I rule a slip of glass with fine black lines, and place it upon the stage; I then rule a disc with black lines and drop it upon the diaphragm of the eye-piece. If the disc be not in focus I turn back the screw of the eye-piece glass, or if this be not sufficient I shift the diaphragm until I get my focus. I then bring the lines on the stage into focus, and parallel with the eye-piece lines. If the objective shall be found to have the usual distortion, it will instantly be seen that although the central stage line is straight and perfectly parallel with, and covered from the top to the bottom of the field by the central eye-piece line, yet the other stage lines bend their ends in a curvilinear direction from the centre of the field. Upon moving the stage, the line that appeared straight assumes the cir-

cular form, and one of the bent lines gets into the centre and assumes its straight appearance, and so on, at every motion of the stage. Upon one occasion, working with a fifth, I was puzzled by a distortion of a kind I could not understand, and a distortion I had never before noticed. Upon resorting to my tests I found the lines bent not from their centre, but straight and parallel through half the lower part of the field, and through the upper three-quarters of the field they spread out like the feathers in the crest of the Prince of Wales. I then knew that the lens (perhaps by a blow or fall) had become displaced, so as to destroy its parallelism.

William Webb.

Read before the Quekett Club, Dec. 27, 1872.

ON "NOBERT'S TESTS."

I MAY be forgiven if I state that astonishment and admiration upon my first examination, under the guidance of the late Mr. Ross, the agent of M. Nobert, and the kind assistance of Mr. Hewitt, of M. Nobert's Tests, betrayed me into an impulsive expression of incredibility and the cry, "Can such things be?" Perhaps my mind was as much impressed as that of any one, and, as a consequence, I worked at the subject with all the ardour of my nature as exhaustively as I was able. At the International Exhibition, 1862, despite the vibration of the gallery in which philosophical instruments were placed, and despite all the surrounding circumstances, I produced about half a dozen coarse specimens after Mons. Nobert. I have related the above bit of egotism simply that you may have a just appreciation of my labour of love. A very short study of the subject produced opinions totally at variance with those of every gentleman who (as far as I know) had expressed himself upon the matter, and that variation of opinion has never been altered, nor have I ever since been in accord with any one gentleman upon the subject. My first proceeding was to ask "What is a line?" My answer was "A line has length and breadth." If a white line be drawn upon a black board [thus] it will be seen that the line is bounded by black sides. To draw another line, the hand must be moved over or past an intervening space of black [thus], so that there shall be a black

boundary to each side of the two lines. The moment that intervening black space is annihilated by drawing a third white line, it becomes self-evident that the three lines have coalesced, and only present one line to the eye. About that I think there can be no controversy.

Having arrived at the conclusion that a line must have a space on each side of it before another line can be drawn, then arose the question, "What is the space between Nobert's lines?" I think it will not be very strong presumption to assume that every microscopist present is familiar with Dr. Jackson's Stage Micrometers, having lines including spaces of the one-thousandth of an inch, or with foreign stage micrometers with hundredths of millimetres in which the spaces are greatly in excess of the width of the lines, and the lines, comparatively coarse, because they are wanted to be used with low powers, with which, if the lines were very fine, they would be invisible. Ten of the lines the thousandth of an inch apart would approximately embrace the field of an eighth of an inch objective with an A eye-piece, as in my specimen numbered 1, to which I shall have again to refer presently. Divide one of those spaces of the one-thousandth of an inch by ten and spaces each of one-ten thousandth of an inch are obtained, as in specimen numbered 2, and this No. 2 is an analogue of Nobert's first band. To divide one of the one-thousandths of an inch by twenty would give spaces the one twenty-thousandth of an inch, as in specimen No. 3. To divide one of the one twenty-thousandths of an inch by ten would give lines each of one 200-thousandths of an inch, of which I have no specimen; and, at this point of the study, I diverge from the beaten path and come to the conclusion that if it be possible to rule lines with clearly defined spaces they can be crossed with similar lines, as in specimen No. 4, where the one four-thousandths are crossed by one four-thousandths, producing squares each one sixteen-millionth of an inch, which would, I believe, enclose the largest human blood corpuscle. In this way lines with spaces the one 200-thousandth of an inch crossing each other would produce squares each the one 40,000-millionth of an inch, or, as the newspapers usually misstate, such a number as the forty-billionth of an inch. I claim to have some knowledge of large figures, as applied to this subject, but the last one, as a ruled square, is beyond my credibility. With all due deference to every gentleman who has studied the subject, I respect-

fully suggest that beyond the first few bands of Nobert's Tests *there is not one containing a line properly so called.* The difference of opinion between gentlemen and myself is so great that I am tempted to state as fully as I am able the reasons of my obstinate tenacity.

If it were possible by fluoric acid, or by other means, to procure a division from side to side—that is to say, across the middle of the bands of one of Nobert's Tests, the vertical section of the first band would present this appearance—

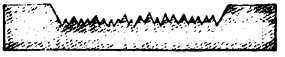
No. 5.



And this is all clear enough—the vertical section would test the Test. Applying this vertical test to the fine bands, quite another state of things will be found to exist. In this last specimen No. 5 has the surface untouched, except by each separate incision. I now advisedly adopt the word incision, for the word line applies no more to these diamond cuttings than it does to the Suez Canal. If the incisions were to be filled with black lead or other opaque substance, the surfaces would become palpable lines.

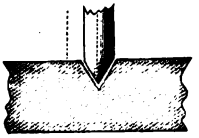
The first few bands would present the same state of things, maintaining the same clearly defined incisions, with intervening surface spaces, the optical effects of which I pass by for the present moment. Upon proceeding beyond the first few bands, and arriving at the fine bands, the vertical section would present the appearance of engraving

No. 6.



which is caused by the tool making contact thus, and moving

No. 6.



laterally a less distance than the extreme width of the incision, almost entirely annihilating the one side of each of the extreme or end incisions of the bands. Each end incision having unequal sides

is most easily proved by focussing for heighth and depth with a moderately high power; but, when I come to the intervening incisions, the matter is complicated by other phenomena. To illustrate this clearly I have prepared the grossly exaggerated specimen No. 8. At this point of the investigation I cannot lay too much stress upon, or too forcibly call attention to, the different appearances of specimens Nos. 1 and 2, as compared with No. 8, upon shifting the focus. With high powers the plane of observation does not include the whole of the depth of the incisions at the same moment—in other words it does not include at the same moment the surface of the glass and the lowest part of the incisions at any one stage of the focussing. The higher the power the less the depth of the plane of observation, or, as is well understood by the expression, the less the penetration. This likewise is easily proved by focussing downwards, and finding the first appearance is that of the upper surface of the glass, with clearly defined holes, which would seem to be continued through the substance; but, upon focussing a little lower, the upper surface is entirely lost to view, and the apparent holes through the glass become greyish black lines. Whence come these coloured lines? The glass is comparatively white! Why do not all the incisions present this dark appearance at the same moment?

At some phases of the focussing of the fine bands one incision will present two black lines. Whence come they? It is necessary to understand something of this phenomenon before proceeding further with the subject. Microscopists are well aware of the polariscopic effects of colour, and of the fact of those colours being produced by refraction, or the bending of the rays of light at particular angles so as to produce only a portion of Fraunhofer's lines—the colour depending upon the particular angle of refraction and the particular portion of the solar spectrum brought under observation. To this polarization of the light by the bending of the rays transmitted through one beveled side of the incision, intersecting, commingling with, and crossing the opposite rays, bent in an opposite direction through the other beveled side of the incision is, very clearly, to my mind, to be attributed these embarrassing black lines. Upon examining specimen No. 9 with the unaided eye, and by powerfully reflected light at a particular angle, the whole of the solar spectrum is brilliantly exhibited, but that is due to the combination of reflection and refraction, while if it be possible to absolutely cut off the

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ay light, and so absolutely destroy the reflection of the top light
even Nobert's Test, the polarized refraction of the transmitted light
would still be present in the black lines. The phenomena of these *black lines become more involved by the fact of the different lengths of the sides of the incisions in the fine bands, scarcely any one incision having its two sides of equal length in a direction from its lowest part to the apices of the ridge on each side of the incision. I say apex because there is no other space dividing the incisions. And these apices are of necessity irregular, because however rigid, however perfect, however true may be the instrument, however capable to a dead certainty may be the projection to the one 200-thousandth of an inch, the very nature of the material worked upon, with the two facts that the diamond has beveled sides and the incision has beveled sides also, create a tendency to elasticity in the machinery and materials, and as an inevitable result the inequality of the ridges, which can only be revealed by this or some analogous test.*

A little familiarity with the phenomena of the black lines prepares one to consider what must be the effect of the refracted ray from the long unbroken side of the outside cut crossing not only the refracted ray from the short side of the cut, but over the first apex and across the two rays from the two sides of the next incision. This complication of the phenomena has produced such a confusion of aerial polarised black lines of light as to embarrass the minds of some gentlemen, and driven them to resort to a declaration of "spectral lines" without giving the slightest hint of their source, and, apparently, wholly unconscious of the remarkable fact that the so-called spectral lines can never interfere with the examination of the incisions, if they were all equal in depth, inasmuch as the depth of the equal incisions and the spectral lines can never with high powers be in focus at the same time as in the equal incisions of the coarse bands where no one of the separate appearances, whether of apparent hole through the substance, or of black line, or of depth, are visible under high powers at the same moment as any one of the other appearances. I am not aware that the expression "spectral lines" has ever been applied to the coarser bands, which may possibly arise from the fact of the operator failing to recognise the dark beauties when arrayed exactly alike and with naught else than their own aerial presence visible at the same time. The production of the irregular polarised black lines I respectfully suggest is an

incontrovertible proof that the diagram numbered 6, with its incisions having unequal sides, and its ridges of unequal heighth, is a correct representation of a vertical section of the fine bands, and the fact of Mr. Slack, after patient skilled labour, despairing of being able to obtain a definition of colloid silica because of this refraction of light, is strongly confirmatory of the accuracy of my views.

After much thought, I have come to the following conclusions, which I now submit, not as absolutely correct, but for the purpose of assisting other students in arriving at their own conclusions. For what they may be worth, I respectfully submit the following—

That a micrometer with lines the one 200-thousandth of an inch apart ruled on glass is an absolute impossibility.

That if it be possible to rule lines themselves of the width of the one 200-thousandth of an inch, to make them definable, there must be a clearly defined line between them, and

A clearly defined line in the same plane of observation.

That beyond the first few coarse bands of M. Nobert's Tests, there is not, properly so called, a single line.

That in the finest bands, except at their extreme sides, there is not half a line.

That in the finest bands the only thing certain, except the edges, is the uncertain polarised aerial lines.

That the microscopical world has been pursuing a phantom, and adopting a fallacy.

That polarisation of light in the examination of these and analogous tests is a deceitful servant of the microscopist.

That oblique illumination is another deceiver.

That if M. Nobert were to attempt to fill his incisions with black, his finest bands would be merged each into one black line of the breadth of each particular band.

That a test must be a known thing which some powers will either disperse or fail to define, as in the case of a spectacle vendor, who places before an intending purchaser's eyes, words printed in types of different sorts as a known test of visual powers.

That there are no tests so reliable as plain opaque lines.

That of plain opaque lines, there are none so reliable as a known measured congeries of contorted lines, as in microscopic writings, where the transmitted rays are partially shut off by the black, and in which the rays transmitted being transmitted by direct illumina-

tion, their definition is not interfered with, such rays becoming parallel rays, passing out at right angles with the surface of the glass, the unalterable law of natural optics being that the angle of incidence and the angle of reflection are equal.

William Webb.

Quekett Journal.

NOBERT'S TESTS AND MR. WEBB.

As I always read the Journal of the Quekett Club with interest, my attention was at once arrested by the communication of Mr. William Webb "On Nobert's Tests," in the July number, in which he arrives at the conclusion "That beyond the first few bands of Nobert's Tests there is not one containing a line properly so called." The mechanical considerations urged by Mr. Webb I will not discuss further than to say that he appears to have overlooked completely one of the most striking facts with regard to Nobert's plates, viz.: That the lines of the first band are not only further apart, but are more deeply ruled than those of the second; that those of the third are still shallower, and so on progressively. This circumstance, it appears to me, destroys his whole argument.

I do not, however, write to discuss Mr. Webb's argument, but to remind the members of the Club that there is a physical reason which compels us to believe that the first fifteen bands, at least, of the nineteen-band plate are composed of real and distinct lines, and that the distance of these lines apart must approximate very closely to what was intended by Nobert.

When the bands of the Nobert's plate are illuminated by oblique light, and are looked at from above with a low power, (too low to show any of the lines), each band appears as a smooth coloured stripe. From the known wave length of the colour seen, and the angle of the incident pencil, the distance which the lines of any band must actually be apart can be computed by the well-known formula for the spectrum of gratings enunciated by Fraunhofer, and the distance thus obtained agrees with that at which Nobert ruled the lines. On the other hand the angle of the incident pencil

being known, and Nobert's given distance being assumed to be true, a table of wave lengths for the different colours may be calculated, and the wave lengths thus deduced agree substantially with those computed by other means. Nobert has discussed the whole subject in two elaborate papers in the 58th volume of *Poggendorff's Annalen* (1852), to which I would refer any who are interested in the mathematical aspects of the question. His discussion leaves, as I think, no room for the possibility of a doubt of the objective reality of the lines up to the fifteenth band.

Now I call attention to the fact that this reason is altogether independent of our ability to resolve the lines with the microscope. In fact, it enabled Nobert to know that his plates were correctly ruled long before the resolution of any but the coarsest bands had been effected by anyone; so that all that Mr. Webb's paper proves is that he does not know how Nobert produces the results, and that notwithstanding his great skill in writing on glass, he cannot do the same thing himself.

As no spectral colour is obtained in the bands finer than the fifteenth, the formula of Fraunhofer cannot be applied to them. In fact, the formula demonstrates that if these bands are actually ruled, as claimed, they can give no spectral colour. For my own part, however, I have no hesitation in expressing the opinion that the four higher bands (16th, 17th, 18th, and 19th) have also an objective reality. I base this opinion upon the comparison of their optical appearances as seen with the best glasses with the appearances of the lower bands (especially those from the 9th to the 15th). These appearances are quite the same in both cases, and as similar results follow similar causes, I infer the existence of real lines in the four higher bands, since I know beyond the possibility of a doubt that they exist in the others. I have discussed the appearances referred to, and the whole matter of the spurious lines which are observed under certain circumstances in connection with the true lines, or instead of them, in the *Monthly Microscopical Journal* for May, 1871. Mr. Webb imagines the real lines also to be spurious, speaks of them as "aërial polarized black lines of light" (whatever that may mean), and talks generally of the part he supposes polarized light to play in the production of the phenomena, in a way which shows his optical notions to be original rather than sound. It is hardly worth while to discuss this part of his paper.

I may mention here, as a matter of interest, that I have recently examined two new Test-plates by Nobert—the first ruled for Professor Barnard, of Columbia College; the second for the Army Medical Museum—in which the maker has attempted to rule lines twice as fine as those of the nineteenth band. These plates have twenty bands. The first ten correspond respectively to the 1st, 3rd, 5th, 7th, 9th, 11th, 13th, 17th, and 19th of the old plate. The lines in the second group of ten bands purport to be ruled at the following distances apart:—The 11th band $\frac{1}{11000}$ of a Paris line, the 12th band $\frac{1}{12000}$, and so on up to the 20th band, lines of which are said to be $\frac{1}{20000}$ of a Paris line apart. As I have not yet been able to resolve any of these new bands I will not at present express an opinion as to whether Nobert has actually succeeded in ruling them as attempted.

Finally, I would say that my attention having been directed to the accounts of Mr. Webb's fine writing on glass, which appears to be almost as marvellous in its way as Nobert's work in its, I have written to Mr. Webb requesting him to prepare a specimen for the Museum. I anticipate both pleasure and instruction from its examination, and have no doubt that I shall find as much to admire in his work as I do to condemn in his arguments.

J. J. Woodward,
U. S. Army.

Quekett Journal, October.

NOTE ON THE SPECTRUM OF CHLOROPHYLL.—M. Chautard (*Comptes Rendus,*) after specifying the changes produced in chlorophyll by light, makes reference to the persistence of green matter in certain plants late in the autumn season, which he considers due to the presence of fatty and resinous matters. He finds that a solution of chlorophyll in fixed oils (oil of belladonna, *e.g.*) is not sensibly altered after several days' exposure in full sunlight. The most luminous spectral rays are the most active in changing chlorophyll solution; and rays which have already traversed a layer of chlorophyll have no effect on a second layer, so long as the first is not discoloured.

ON WEBB'S TEST, AND OTHER FINE WRITING ON GLASS.

THE Army Medical Museum has just received from William Webb, of London, two samples of his fine writing on glass, intended to serve as test-objects for high-power objectives. In each the writing is on the under surface of a thin glass cover, mounted on an ordinary glass slide, three inches long by one wide, and the slides are in every respect duplicates, except that in one the writing has been blackened, and the cover cemented to the slide by Canada balsam, while in the other the writing is not blackened, and the cover is merely secured by a ring of cement, so that a stratum of air intervenes between the writing and the slide.

The inscription on both covers is identical, and is arranged as follows:

Written with a Diamond, by Wm. Webb,
London, England.
To the order of Dr. J. J. Woodward.

Our Father which art in
heaven hallowed be Thy
name Thy kingdom come
Thy will be done on earth
as it is in heaven Give us
this day our daily bread and
forgive us our trespasses as we
forgive them that trespass
against us and lead us not
into temptation but deliver
us from evil Amen.

For the Army Medical Museum,
Washington, D. C.

Both glass slides are labelled with the following words, boldly written with a diamond, so as to be quite legible to the naked eye:

"Webb's Test—The Lord's Prayer. 227 letters in the $\frac{1}{254} \times \frac{1}{441}$ of an inch, or the $\frac{1}{129,654}$ of a square inch, and at the rate of 29,431,458 letters to an inch, which is more than 8 Bibles, the Bible containing 3,566,480 letters."

These slides were prepared by Mr. Webb in response to a letter I addressed to him, August 18, 1873, in which I requested him to "prepare for the Museum such a specimen of your fine writing as you may regard best calculated to exhibit your skill in this direction." The price paid for the two was twenty pounds sterling.

On measuring the writing I find that the whole inscription, on each cover, occupies a square, with a side of $\frac{1}{75}$th of an inch. The

Prayer, which is in much finer writing than the rest of the inscription, occupies a somewhat irregular parallelogram; not only no two lines are of the same length, but the commencing letters of the several lines are not exactly one above the other, so that a line drawn through them describes a slight, somewhat irregular curve. In measuring this parallelogram I took the length of the several lines, and found the mean to be 33½ ten-thousandths of an inch; from the top of the capital letters of the first line to the bottom of the last line, I made 22½ ten-thousandths of an inch. The measurements were made with an eighth, the draw-tube being so adjusted that each division of a glass eye-piece micrometer, ruled to two hundred and fiftieths of an inch, represented the twenty-thousandth part of an inch. The readings were 67 divisions in one direction, 45 in the other. The result corresponds very closely with Mr. Webb's inscription, but is a trifle smaller in each direction, for $\frac{1}{294}$th of an inch = .003402″+, and $\frac{1}{441}$th of an inch = .002267″+.

Examined with a suitable objective the writing is perfectly legible, but as might be anticipated has a crowded appearance, the space between the lines being less than the height of the letters, and not exactly the same between any two lines, and the distance between the words being proportionally less than is usual in ordinary writing. It may also be remarked that the t's are of the form which does not require to be crossed; that the h's, l's, and b's are without loops; that the i's are not dotted, and that there are no punctuation marks.

I could not avoid being struck with the extreme similarity between the chirography of the prayer on the blackened balsam-mounted, and the unblackened dry slide,* not merely in the dimensions of the writing, the number of words in each line, and the characteristic peculiarities of form of each letter, but in the relative distances between corresponding lines and words, and in all the irregularities of arrangement — the one plate is the fac-simile of the other; and I cannot even say that I found the one in which the writing was blackened easier to read than the one in which it was not. I found, indeed, no difficulty in reading either with a half-inch objective, though, of course, higher powers were useful, and indeed were necessary to enable me to form an opinion of the character of the mark made by the writing diamond.

*This similarity would readily be attainable for an indefinite number of slides, by such machines as are used for microscopic writing.

The glass covers on which the writing was inscribed were, however, too thick for the advantageous use of the highest powers. For example, the immersion $\frac{1}{16}$th, of Powell & Lealand, would not work through either.

I may also mention, as of interest to any one who may feel tempted to order similar slides, that both those received at the Museum came to hand broken, in consequence of the imperfect manner in which they were packed. The fractures, fortunately, however, did not involve the covers, so that the writing remained uninjured, notwithstanding the misfortune.

My attention had been particularly directed to Mr. Webb's fine writing, by a paper of his on "Nobert's Tests,"* published in the *Journal of the Quekett Microscopical Club*, for July, 1873, in which he undertook to deny that the lines in any but the coarsest bands of Nobert's plates have any real existence; attempted to show that it was mechanically impossible, that lines so close as Nobert affirms the lines in his finer bands to be, could be ruled by a diamond; and accounted for the lines actually seen in these bands with high powers, by attributing them to the polarization of light!!! The paper concluded by claiming "That there are no tests so reliable as plain, opaque lines," and "that of plain, opaque lines, there are none so reliable as a known measured congeries of contorted lines, as in microscopic writings."

After reading this paper, I felt it my duty to address to the Quekett Club a communication,† which will be found in the October Number of their Journal, in which I briefly called attention to the physical considerations which enable us to know certainly, and even without examining them with the higher powers of the microscope, that the bands ruled by Nobert, as far, at least, as the fifteenth band of the nineteen-band plate are composed of actual lines, at a distance apart which must approximate very closely to that assigned, and referred to Nobert's papers, in the 58th volume of *Poggendorff's Annalen* (1852), in which the mathematical aspects of the question are satisfactorily discussed. I further stated that although these physical conditions are not applicable to the highest four bands, (since they display no color when obliquely illuminated,) yet the optical appearances of these bands, when studied with objectives of adequate

* Page 216, *ante, et seq.* † Page 222, *ante, et seq.*

power, are so similar to those of the lower ones, that it seems unreasonable to doubt that they also are ruled as stated by Nobert.

Mr. Webb's paper also induced me to address to him the request for a sample of his finest writing, in response to which I received the slides above described. I will institute no comparison between these slides and the rulings of Nobert, as works of art, the two being so very different, and each being marvellous in its way. But I cannot admit that Mr. Webb's writing has any such value as a test-object, as is possessed by Nobert's plates. The work of the diamond in Mr. Webb's writing corresponds in coarseness very nearly to the rulings in the third band of Nobert's plate, a conclusion I have arrived at after careful study with the highest powers I could employ on the writing, and after making photographs of the writing and of the lower bands of the plate with the same power, I find that the writing is legible with objectives which will only imperfectly resolve the ninth band of the plate, and will not show the lines in any of the finer bands. Of course it can be used as a test-object for medium powers, but it can be satisfactorily displayed by objectives which cannot be made to show the striæ on *Amphipleura pellucida* or *Frustulia Saxonica*, which will not show *Pleurosigma angulatum* in beads, or even give a distinctly beaded appearance in *Pleurosigma formosum*.

Mr. Webb tells us in his paper that he has in vain endeavored to produce ruling on glass, as fine as even the medium bands of Nobert's plate. An examination of his writing will, I think, explain the reason why. The diamond point with which he writes, pressed into the glass as he uses it, would hardly produce separate lines as fine as the fifth band of the plate. Nobert must use very different points, and handle them very differently, indeed.

Mr. Webb has had several distinguished predecessors in the art of executing microscopic writing on glass. The first of these, so far as I have been able to ascertain, was Froment of Paris, of whom Lardner tells us in his little book on the microscope*, that on the occasion of the Great Exhibition in 1851, he engraved on glass, in a circle the $\frac{1}{80}$th of an inch in diameter, the Coat of Arms of England—lion, unicorn and crown,—with the following inscription, partly in Roman letters, partly in script: "*Honi soit qui mal y*

*The Microscope, by Dionysius Lardner, London, 1856.

pense, Her Most Gracious Majesty, Queen Victoria, and His Royal Highness Prince Albert, *Dieu et mon droit.* Written on occasion of the Great Exhibition, by Froment, à Paris, 1851." Lardner gives a wood cut, representing this piece of engraving as seen with 120 diameters, and also a cut representing a less complicated subject, engraved by the same artist for himself; but says that he is not at liberty to explain the details of the method by which the results were produced.

The microscopic writing and engraving of Froment attracted the attention of Mr. N. Peters, a banker of London, who thereupon devised a machine, with which he produced writing still more minute. Mr. R. J. Farrants read before the Microscopical Society of London, April 25, 1855, an account of this machine, from which I quote the following: "The result is a machine capable of executing and recording movements of almost inconceivable minuteness. With it, in its present condition, Mr. Peters has written the 'Lord's Prayer,' (in the ordinary writing character, without abbreviation or contraction of any kind,) in a space not exceeding the one hundred and fifty-thousandth of a square inch. There are, in this specimen, six lines of writing; the length of the sides of a parallelogram, to include the whole, would be $\frac{1}{250}$th and $\frac{1}{600}$th of an inch, linear."

I refer the curious reader to Mr. Farrants' lucid description of the Peters' machine, which is illustrated by several wood cuts.* As first devised it consisted essentially of a single, tolerably heavy, lever, suspended vertically; the lower or long arm of the lever being connected by suitable devices with a pen or pencil, while the upper short arm bore the diamond point, with which the fine writing was to be executed. Any motion of the lower pencil would be repeated in the opposite direction by the diamond point, in a scale diminished as many diameters as the one arm of the lever was shorter than the other. Mr. Peters subsequently improved his machine, by substituting for the simple lever a compound one, by the various adjustments of which, a range of diminutious from 110 to 6,250 diameters, could be attained at pleasure.

In February, 1862, Mr. Farrants, as President of the Microscopical Society,† again alluded in some detail to the Peters' machine, stating that since he had read his paper in 1855, some further

*Transactions Microscopical Society, London, Vol. III, 1855, p. 55. † *Ibid.* Vol. X, 1862, p. 69.

improvements had been made, the chief of which was that the diamond point was now stationary, while the glass to be written on was moved by the levers. As a result of these improvements still finer writing had been executed, of which Mr. Farrants gives the following account:

"The Lord's Prayer, too, has been written, and may be read in $\frac{1}{856,000}$th of an English square inch. The measurements of one of these specimens were verified by Dr. Bowerbank, with a difference of not more than one five-millionth of an inch, and that difference, small as it is, arose from his not including the prolongation of the letter 'f' in the sentence 'Deliver us from evil,' so that he made the area occupied by the writing less than that stated above. Some idea of the minuteness of the characters in these specimens may be obtained from the statement that the whole Bible and Testament, in writing of the same size, might be placed twenty-two times on the surface of a square inch. The grounds for this startling assertion are as follows: The Bible and Testament together, in the English language, are said to contain, 3,566,480 letters. The number of letters in the Lord's Prayer, as written, ending with the sentence, 'Deliver us from evil,' is 223,* whence as $^{3,566,480}/_{223} = 15,992$, it appears that the Bible and Testament together contain the same number of letters as the Lord's Prayer written 16,000 times; if, then, the prayer was written in $\frac{1}{16,000}$th of an inch, the Bible and Testament, in writing of the same size, would be contained by one square inch; but as $\frac{1}{856,000}$th of an inch is less than the $\frac{1}{22}$d part of $\frac{1}{16,000}$d of an inch, it follows that the Bible and Testament, in writing of that size, would occupy less space than $\frac{1}{22}$d of a square inch; in other words, the writing is so small that in similar characters, the Bible and Testament together could be written twenty-two times in the space of one English square inch."

If these statements of Mr. Farrants are accurate, and I know at present of no reason for thinking the contrary, the writing by Mr. Peters was several times finer than that of Mr. Webb. I hope at some future time to be able to see a sample of his work, which I should examine with much interest, being especially desirous of observing how it compares with that of Mr. Webb, as to the qualities of neatness and beauty, as well as in minuteness.

*Mr. Webb includes the word "Amen," four letters, hence makes 227 letters in the Prayer.

I note in Dr. Barnard's report* of the Paris exposition of 1867, that Mr. E. Hardy, of Paris, exhibited there a machine for executing fine writing on glass, but exact data as to its performance are not given in the report. It is spoken of, however, as less perfect than the Peters' machine, Mr. Farrants' description of the performance of which is quoted.

I ought not to close this paper without referring to the micro-ruling on glass and steel of Mr. John F. Stanistreet, of Liverpool, an interesting account of which will be found in a paper by Mr. Henry J. Slack, in the *Monthly Microscopical Journal* for September, 1871. In December, 1871, the same journal published a drawing and description of the machine, by Mr. Stanistreet, himself, who concludes his article as follows: "The machine is constructed for ruling lines from $\frac{1}{1,000}$th to the $\frac{1}{10,000}$th of an inch apart, and I have added to it the means of further subdivision to the $\frac{1}{100,000}$th of an inch; but I have not yet been able to procure any diamond fine enough for ruling distinctly more than about 5,000 lines per inch."

It is indeed, in this matter of the point for ruling, that those who have attempted to imitate the Nobert's plate have broken down. The Peters' machine, according to Mr. Farrants, could have ruled lines as fine as those of Nobert, if a suitable diamond point could have been obtained. How Nobert produces such diamond points as he must use, if, indeed, his points are diamonds, he has never told, but still keeps his whole method secret. It is to be hoped that when his useful career comes to an end, this ingenious mechanic and optician, will not allow the secret to perish with him, but will select his own time and method of communicating it to the scientific world.

J. J. Woodward,
Assistant Surgeon, U. S. Army.

Washington.

*Paris Universal Exposition, 1867. Reports of the United States Commissioners. Machinery and Processes of the Industrial Arts, and Apparatus of the Exact Sciences, by F. A. P. Barnard, LL. D., Washington, Government Printing Office, 1869.

THE DIATOMACEÆ IN THE SOUNDINGS OF THE EXPEDITION FOR THE EXPLORATION OF THE BALTIC.

The various species of Diatomaceæ noted in this paper were found in the soundings made by the Expedition for the Exploration of the Baltic Sea, by His Majesty's Steam Corvette "Pommerania," in the Summer of 1871. The soundings were studied by Mr. J. H. L. Flögel, and the present paper is a translation of a portion of his notes, which were published at Kiel, the past Summer.

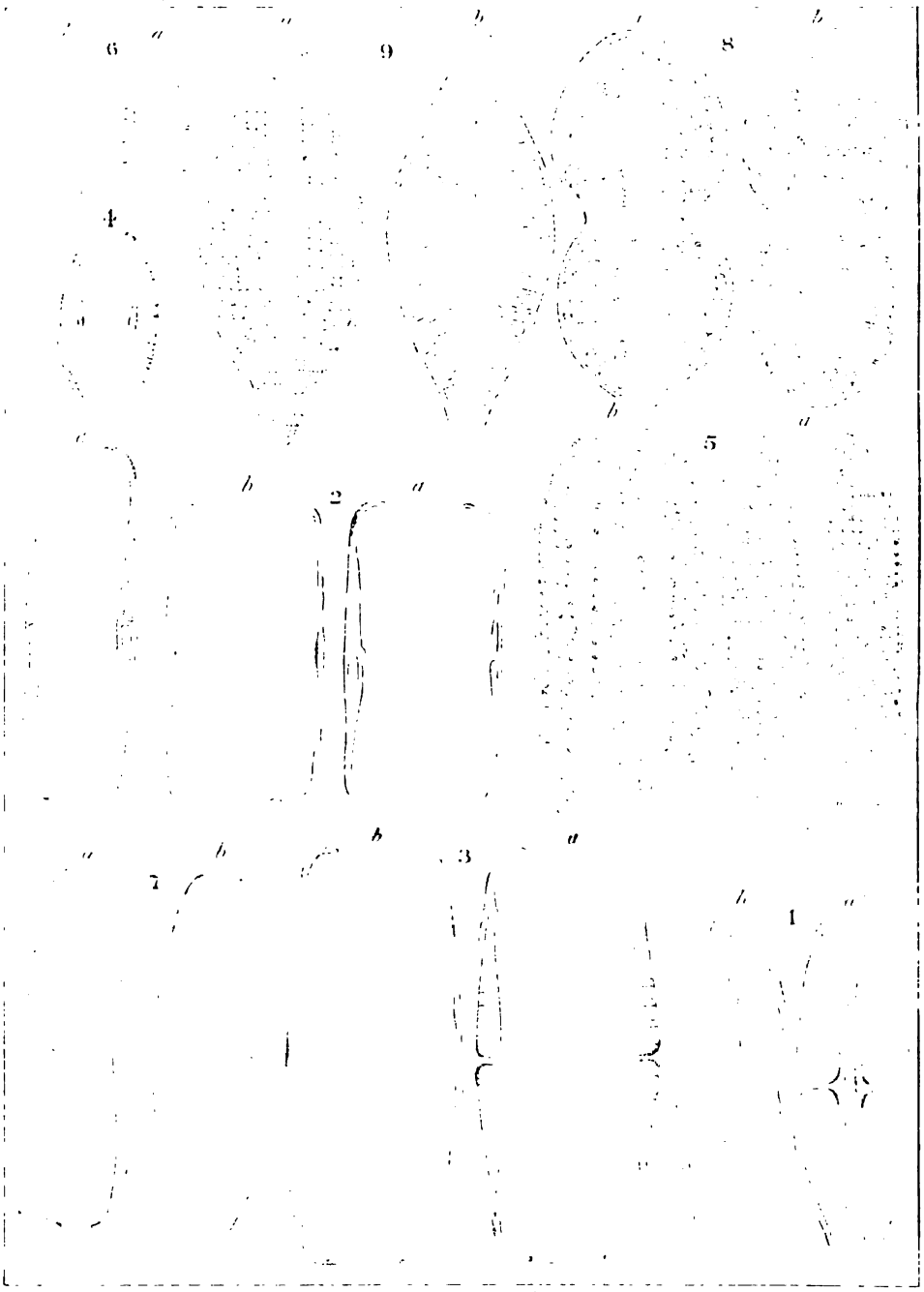
The soundings, shortly after being taken, were placed in alcohol. In most cases the specimens appear to have been already lifeless when gathered; the protoplasm was decomposed; the frustules were frequently parted, and the mucous sheaths and pedicles were very generally unrecognizable. It has, therefore, been impossible to identify with certainty some of the forms found.

The greater portion of the soundings contained no Diatomaceæ. Only one of the gatherings from the Kattegat was very rich. This was taken from the east coast of Jutland, near Laesoe island. The soundings from the harbor of Arendal were also very prolific. In the others we find only isolated shells, of but few species.

It is worthy of note that some of the commonest species (*Achnanthes, Synedra, Melosira, &c.*), which grow parasitic upon the larger algæ in the Baltic harbors, are either very sparsely represented in the soundings, or are not found there at all. There must here exist conditions, yet unexplained, which regulate the depths reached by these forms. Indeed, as respects animal life, Häckel (*Biologische Studien*, S. 93,) has already called attention to the fact that animals peculiar to deep water are never found in silt.

On the other hand, it is very remarkable, that numerous rare forms are found in the Kattegat soundings, which, so far as known, have hitherto only been noted on the west coast of Scotland. This is probably due to the arm of the Gulf Stream which, passing through the Kattegat, transfers these forms to the place where they

NOTE.—The classification adopted in this paper by the translator, is that of Prof. H. L. Smith. In the original the classification of Grunow is followed. All the figures on the plate have a magnification of 970 to 1000 linear.



From Soundings in the Baltic Sea.

are found. Some of these rare forms (*Amphoræ,*) have been noted in the waters of Spitzbergen. (*Vide* Cleve, *Diatomaceer fran Spetsbergen*, 1867.)

Examples of each species herein noted have been mounted in balsam, and properly catalogued, so that if any errors in identification have been made they may be corrected hereafter.

Family I. CYMBELLEÆ. H. L. S.

Gen. 1. Amphora. Ehr.

a. Nodulis transverse elongatis, non rotundatis.

1. *A. lævissima.* Gregory, Diat. Clyde, f. 72; Rabenhorst, Flora Algarum, S. 87.
 Knarrhoi, (on the coast of Jutland, near Laesoe island.)
2. *A. lævis.* Greg. D. C., f. 74.
 Knarrhoi.
2. *A. acuta.* Greg. D. C., f. 93.
 Arendal harbor.
4. *A. hians* n. sp. (*ad interim.*)
 A. elliptica, apicibus truncata, valvis angustis, utroque polo acuminatis et illic hiantibus, medio leviter constrictis, nodulo transverse percurso, striis inconspicuis, latere dorsali lævi (non logitudinaliter) lineato. Length, 45 μ. Breadth, 15 μ.
 Knarrhoi. (Pl. IV, f. 1, a, *venter*; b, *dorsum.*)
5. *A. parallela,* n. sp. (*ad int.*)
 Mediocris, rectangularis, polis truncatis; valvis subplanis; nodulis centralibus transversis longe distantibus, brevissimis; striis indistinctis; latere dorsali longitudinaliter lineato. Length, 45 to 53 μ. Breadth, 16 to 22 μ.
 Knarrhoi. (Pl. IV, f. 2, a. b. c.)
6. *A. Nobilis* (?). Greg. D. C., f. 87.
 Resembles this diatom very closely, though varying somewhat from Gregory's figure here cited. The valves are a little broader in the middle; the lines upon the ventral side are somewhat curved, more resembling indeed *A. Arcus.* Length 57 μ. Breadth 28 μ.
 Knarrhoi. (Pl. IV, f. 3.)

b. Nodulis rotundis.

a. Apicibus rostratro-porrectis.

7. *A. lineata.* Greg. D. C., f. 70.
Knarrhoi.

8. *A. granulata.* Greg. D. C., f. 96.
Knarrhoi.

9. *A. ventricosa.* Greg. D. C., f. 68.
Knarrhoi.

β. Apicibus non rostratis.

10. *A. oblonga* (?). Greg. D. C., f. 68.
A form which might well be referred to *A. Proteus*, except that I do not find the characteristic striæ of the dorsal side.
Knarrhoi.

11. *A. Grevilliana.* Greg. D. C., f. 89.
Knarrhoi.

12. *A. proteus.* Greg. D. C., f. 81.
Arendal harbor, scarce; Knarrhoi, abundant.
Variety *parvula.* (Pl. IV, f. 4.)
Knarrhoi.

13. *A. sulcata.* Bréb. Greg. D. C., f. 92.
Arendal harbor, 30 fathoms.

14. *A. dubia.* Greg. D. C., f. 76.
Knarrhoi.

15. *A. crassa.* Greg. D. C., f. 94.
A beautiful form which is also found at Spitsbergen (*vide* Cleve). Distance of the punctæ, 1.5 to 1.6 μ. In Gregory's figure these are not accurately drawn. (Pl. IV, f. 5.)
Knarrhoi.

16. *A. spectabilis.* Greg. D. C., f. 80.
Knarrhoi.

17. *A. arcus.* Greg. D. C., f. 88.
Knarrhoi.

18. *A. nana.* Greg. D. C., f. 64.
Only 25 μ in length, and 10 μ in breadth. Striæ, 0.65 μ. Might easily be confounded with *A. proteus*, var. *parvula*, (*ante.*)
Knarrhoi. (Pl. IV, f. 6.)

19. *A. angusta.* Greg. D. C., f. 66.
Knarrhoi.

20. *A. bacillaris* (?). Greg. D. C., f. 100.
This form agrees well with Gregory's figure, but the striation is much coarser, namely: 0.8 μ. Length, 43 μ; breadth, 13 μ.
Knarrhoi.

21. *A. excisa* (?). Greg. D. C., f. 86.
Knarrhoi, one specimen only.

22. *A. tenuis*, n. sp. (*ad int.*)
Mediocris, elliptico-oblonga, polis truncatis; valvis convexis, ventre striato, nodulo rotundato, linea media parum curvata, dorso longitudinaliter lineato. Length, 52 μ. Breadth, 16 μ.
Knarrhoi. (Pl. IV, f. 7.)

Gen. II. CYMBELLA. H. L. S.

23. *C.* (*cocconema*) *Boeckii* (?). Ehr. K. B. T. 6, f. 5. S. B. D. f. 233. ? Rab. F. A. S. 83.
The single specimen found seems properly to be referred to this species. It agrees better with the figure of Kützing than that of Smith. The striæ, 1.46 μ (17 in .001″ English), run regularly at right angles to the length, leaving a free fine median line in which there is no appearance of a nodule.
Skagen. 110 fathoms.

FAMILY II. NAVICULEÆ. H. L. S.

Gen. I. STAURONEIS. Ehr.

1. *S. pulchella.* W. S. B. D., f. 194.
Arendal, very abundant.

Gen. II. NAVICULA. Bory.

2. *N. subtilis.* Greg. D. C., f. 19.
Resembles *P. subtilis*, Gregory D. C., *loc. cit.* I found only one specimen.
Knarrhoi.

3. *N. Lyra.* Ehr. Greg. D. C., f. 13 and 14.
Arendal, Knarrhoi, abundant.

4. *N. forcipata.* Grev. Rab. F. A. 178.
Arendal, one specimen.

5. *N. Hennedyi.* W. S. B. D. II, p. 93.
Arendal, abundant.

6. *N. Sandriana.* Grun. Hedwigia, 1864, S. III.
Arendal, one specimen.

7. *N. Smithii.* Bréb. S. B. D., f. 152 a.
Arendal, Knarrhoi, very abundant.

8. *N. fusca.* Pritch.
Resembles *N. Smithii* var. *β. fusca.* Greg. D. C., f. 15.
Arendal.

9. *N. suborbicularis.* Greg. D. C., f. 17. *N. Smithii,* variety *suborbicularis.*
Arendal, Knarrhoi, abundant.

10. *N. Liber.* W. S. B. D., f. 133.
Arendal, Knarrhoi, not unfrequent.

11. *N. elegans.* W. S. B. D., f. 137.
Arendal.

12. *N. palpebralis.* Bréb. S. B. D., f. 273.
Knarrhoi.

13. *N. inconspicua.* Greg. D. C., f. 3.
Knarrhoi, one specimen only.

14. *N. quadrata.* Greg. Rab. F. A. S. 201.
Arendal, Knarrhoi.

15. *N. didyma.* Ehr. Kütz. Bac., T. 4, f. 7. S. B. D., f. 154 a.
Besides the normal form, I noted one remarkably small specimen, which was only 48 μ in length, and 18 μ in width, with a striation of only 0.83 μ, (31 in .001″). One specimen (*see the figure,*) shows a remarkable malformation. One valve is strictly normal, but in one half the other the punctæ radiate from the centre in peculiar curves.
Arendal, Knarrhoi, abundant. (Pl. IV., f. 8.)

16. *N. Bombus.* Ehr. G. D. C., f. 12.
Arendal, Knarrhoi, scarce.

17. *N. Pandura.* Bréb. Rab. F. A., S. 205 and 219.
Resembles *Pinnularia Pandura,* Greg. D. C., f. 22.

A magnificent form of which I found only one specimen.
Arendal.

18. *N. bicuneata.* Grun. Rab. F. A., S. 206.
Arendal.

19. *N.* (*Pinnularia*) *distans* W. S. B. D., f. 169.
Arendal, very abundant.

20. *N.* (*Pinnularia*) *stauroptera* (?). Grun.
Resembles *Stauronis parva.* Kütz. Bac. T. 29, f. 23. I am more in doubt as to the identity of this form because *P. stauroptera* is a fresh water diatom.
Knarrhoi, one specimen only.

Gen. III. AMPHIPLEURA. Kütz.

21. *A. sigmoidea.* W. S. B. D., f. 128 b.
Arendal, Knarrhoi, not unfrequent.

Gen. IV. PLEUROSIGMA. W. S.

22. *P. fasciola.* Ehr. S. B. D., f. 211.
Knarrhoi, abundant.

23. *P. prolongatum.* W. S. B. D., f. 212.
Knarrhoi, one specimen.

24. *P. strigosum.* W. S. B. D., f. 203.
Arendal, very abundant; Knarrhoi, not unfrequent.

25. *P. naviculaceum* (?). Bréb. Rab. F. A., S. 233.
The same form which J. D. Möller publishes as *P. naviculaceum*, but I am doubtful of its identity.
Arendal, Knarrhoi.

26. *P. obscurum.* W. S. B. D., f. 206.
Knarrhoi.

27. *P. angulatum.* Quekett. S. B. D., f. 205.
Arendal, Knarrhoi, Skagen, abundant.

Gen. V. AMPHIPRORA. Ehr.

28. *A. vitrea.* W. S. B. D., f. 270.
Knarrhoi.

29. *A. alata.* Ehr. K. B., T. 3. f. LXIII, S. B. D., f. 124.
Knarrhoi, abundant.

Gen. VI. SCHIZONEMA. Agardh.

30. *S. crucigerum.* W. S. B. D., f. 354.
Knarrhoi.

FAMILY III. GOMPHONEMEÆ. H. L. S.

Gen. I. RHOICOSPHENIA. Grun.

1. *R. curvata.*
Resembles *Gomphonema curvatum.* Kütz. Bac. T. 8., f. 1.
Knarrhoi, scarce.

FAMILY IV. COCCONIDEÆ. H. L. S.

Gen. I. COCCONEIS. Ehr.

1. *C. scutellum.* Ehr. Kütz. Bac., T. 5, f. VI, 3 and 6. S. B. D., f. 34.
Knarrhoi, abundant; not unfrequent in other localities.

2. *C. consociata.* Kütz. K. B., T. 5, f. VIII, 6.
Arendal, abundant; Knarrhoi, Skagen.

3. *C. pygmæa.* Kütz. K. B., T. 5, f. VI, 4.
Knarrhoi.

4. *C.* (?) *danica.* n. sp.
Major, elliptico-rhombea, apicibus acutis: Valva superior:—striis latissimus, rectis, distantibus; linea media recta lata, nodulo centrali nullo. Valva inferior:—striis multo tenuioribus, rectis, circa nodulum vix radiantibus, linea media recta ut in superiori; nodulo centrali transverse dilatato.
Length 62 μ, breadth 26 μ. Striation of the upper valve, 1.6 μ, of the lower 0.6—0.7 μ.
Knarrhoi. (Pl. IV, f. 9.)

FAMILY V. FRAGILAREÆ. H. L. S.

Gen. I. EPITHEMIA. Bréb.

1. *E. Hyndmanii.* W. S. B. D., f. 1.
Knarrhoi, only.

Gen. II. PLAGIOGRAMMA. Grev.

2. *P. Gregorianum.* Grev.
Resembles *Denticula staurophora.* Greg. D. C., f. 37.
Knarrhoi, quite abundant.

Gen. III. DIMEREGRAMMA. Pritch.

3. *D. nanum.* Pritch.
Resembles *Denticula nana.* Greg. D. C., f. 34.
Found of all sizes and in numberless quantity, mixed with sand, at Knarrhoi; not unfrequent at Arendal.

Gen. IV. RAPHONEIS. H. L. S.

4. *R.* (*Doryphora*) *Amphiceros.* Ehr. Kütz. Bac., T. 5, f. 10; T. 21, f. 2; S. B. D., f. 224.
Arendal, 30 fathoms, scarce.

Gen. V. SYNEDRA. Ehr.

5. *S. parva.* Kütz. Bac. T. 15, f. 9.
Arendal, 30 fathoms.

6. *S. crystallina.* Kütz. Bac. T. 16, f. 1. S. B. D., f. 201.
Arendal.

7. *S. fulgens.* W. S. B. D., f. 103.
Licmophora fulgens. K. B., T. 13, f. 5.
Arendal.

8. *S. affinis.* Kütz. Bac. T. 15, f. v and xi, and T. 24, f. 1, 5. S. B. D., f. 97.
Knarrhoi.

FAMILY VI. TABELLARIEÆ. H. L. S.

Gen. I. GRAMMATOPHORA.

1. *G. subtillissima.* Schact. Rab. F. A., S.304.
Like *G. Oceanica.* Bailey.
Arendel, Knarrhoi, very abundant.

2. *G. angulosa.* Ehr. K. B., T. 30, f. 79.
Knarrhoi, unfrequent.

3. *G. serpentina.* Ralfs. K. B., T. 29, f. 82. S. B. D., f. 315.
Knarrhoi.

Gen. II. RHABDONEMA. Kütz.

4. *R. arcuatum.* Lyng. K. B., T. 18, f. 6. S. B. D., f. 305.
Bornholm, Arendal, unfrequent.

5. *R. minutum* (?). Kütz. K. B., T. 21, f. 11, 4. S. B. D., f. 306.
Arendal.

FAMILY VII. SURIRELLEÆ. H. L. S.

Gen. I. TRYBLIONELLA. W. S.

1. *T. punctata.* W. S. B. D., f. 261 and 76 a.
 Knarrhoi.
2. *T. constricta.* Greg. S. B. D., II, p. 89.
 Knarrhoi.

Gen. II. SURIRELLA. H. L. S.

3. *S. fastuosa.* Ehr. K. B., T. 28, f. 19 a–d. S. B. D., f. 66.
 Arendal, unfrequent; Knarrhoi, abundant.
4. *S.* (*Campylodiscus parvulus*) *parvula* W. S. B. D., f. 56.
 Arendal, Knarrhoi, unfrequent.

Gen. III. NITZSCHIA. H. L. S.

5. *N. constricta.* (Kütz.) Pritch.
 Synedra constricta. Kütz. Bac. T. 3, f. 70.
 Nitzschia dubia. W. S. B. D., f. 112.
 Knarrhoi, scarce.
6. *N. hyalina.* Greg. D. C., f. 104.
 Knarrhoi.
7. *N. angularis.* W. S. B. D., f. 117.
 Knarrhoi.
8. *N. scalaris* (?). W. S. B. D., f. 115.
 Synedra scalaris. Ehr.
 Pritchardia scalaris. Rab. F. A., S. 162.
 Knarrhoi.
9. *N. paradoxa.* H. L. S.
 Bacillaria paradoxa. Gmel. S. B. D., f. 279.
 Knarrhoi.

FAMILY VIII. MELOSIREÆ. H. L. S.

Gen. I. MELOSIRA. Agardh.

1. *M. nummuloides* (?). Ag. Kütz. B., T. 3, f. 3. S. B. D., f. 329.
 Arendal, Knarrhoi, not unfrequent.
2. *M. hormoides.* H. L. S.
 Podosira homoides. Mont. K. B., T. 29, f. 84. S. B. D., f. 327.
 Arendal harbor, not unfrequent.

3. *M. marina.* H. L. S.
 Paralia marina. Heiberg.
 Resembles *Orthosira marina.* W. S. B. D., f. 338, and *Melosira sulcata.* (Ehr.) K. B., T. 2, f. 7.
 The most abundant of all the Diatomaceæ in nearly all the soundings. It appears to favor the deepest regions of the sea.
 Southern Norway, in the channel, 367 fathoms deep, it is almost the only diatom; Skagen, 110 fathoms; Ronehamn, 120 fathoms; Bornholm, &c., &c.

FAMILY IX. BIDDULPHIEÆ. Kütz.

Gen. I. BIDDULPHIA. H. L. S.

1. *B. aurita.* Lyngb. S. B. D., f. 319. K. B., T. 29, f. 88.
 Arendal, Knarrhoi, very abundant and of all sizes.

2. *B. rhombus.* Ehr. S. B. D., f. 320.
 Zygoceras rhombus. K. B. T. 18, f. IX.
 Arendal.

3. *B. Baileyi.* W. S. B. D., f. 322.
 Arendal, Knarrhoi, not unfrequent.

4. *B. striolatum.* H. L. S.
 Triceratium striolatum. Ehr. K. B. T. 18, f. 10.
 Arendal.

FAMILY X. EUPODISCEÆ. H. L. S.

Gen. I. AULISCUS. Ehr.

1. *A. sculptus.* W. S. B. D., f. 42.
 Arendal mud, not unfrequent; Knarrhoi, more abundant.

Gen. II. EUPODISCUS. Ehr.

2. *E. radiatus* (?). Bailey. S. B. D., f. 255.
 Arendal, one specimen.

FAMILY XI. HELIOPELTEÆ. H. L. S.

Gen. I. ACTINOPTYCHUS. Ehr.

1. *A. undulatus.* Kütz. Bac., T. 1, f. 24. S. B. D., f. 43.
 Of all sizes, abundant in mud from Arendal, Knarrhoi, and at Skagen at a depth of 110 fathoms.

FAMILY XII. COSCINODISCEÆ. H. L. S.

Gen. I. CYCLOTELLA. Kütz.

1. *C. Kützingiana.* Thw. S. B. D., f. 47.
Knarrhoi, one specimen.

Gen. II. ACTINOCYCLUS. Ehr.

2. *A. Ehrenbergii.* Pritch. Rab. F. A., S. 6, f. 6.
Arendal harbor.

Gen. III. COSCINODISCUS. Ehr.

3. *C. radiatus.* Ehr. K. B., T. 1, f. 18. S. B. D., f. 37. Rab. F. A., S. 6.
Numberless in the soundings of Arendal harbor; not unfrequent at Skagen, 110 fathoms deep; Knarrhoi; Bornholm.

4. *C. Oculus Iridis.* Ehr. Microgeol, T. 18, f. 42; T. 19, f. 2.
Arendal, adundant; Knarrhoi, not unfrequent, in fragments.

5. *C. excentricus.* Ehr. K. B., T. 1, f. 9. S. B. D., f. 36.
Arendal harbor; Knarrhoi, abundant.

6. *C. lineatus.* Ehr. K. B., T. 1, f. 10.
Knarrhoi, not unfrequent.

7. *C. minor.* Ehr. K. B., T. 2, f. 12, 13. S. B. D., f. 36.
Arendal harbor, 30 fathoms.

8. *C. flavicans* (?). Ehr. K. B., T. 28, f. 8.
Arendal harbor.

9. *C. concinnus.* W. S. B. D. II, S. 85.
Arendal harbor, 30 fathoms.

10. *C. centralis.* Ehr. Greg. D. C., f. 49.
Arendal harbor.

S. A. Briggs.

Chicago.

THE RELATIVE PRICES OF ENGLISH AND AMERICAN OBJECTIVES.

IN the August number of the *Monthly Microscopical Journal* appeared a paper, by Dr. Pigott, in which is the following passage: "I have very little doubt, that if any one be willing to offer Messrs. Powell & Lealand double the price of their $\frac{1}{16}$th, the same as charged for Tolles' immersion $\frac{1}{18}$th by Mr. Stodder, $175, or £34 sterling, they would be able to produce a glass proportionately improved in some of the minor details." Dr. Pigott has, of course, unintentionally, made a large mistake in the comparative prices of the two instruments—a mistake that, in my experience, "uncommercial" writers have too often made, from not knowing the condition of United States currency—and he has made a wrong comparison from not knowing what was sold to Dr. Woodward.

These errors are calculated to do a serious pecuniary injury to Mr. Tolles. Dr. Pigott (evidently) values the £ sterling at $5 U. S. Currency. The actual value to-day is $5.60. (When Dr. Pigott was writing it may have been ten to twenty cents more), so that the price $175, paid by Dr. W., was not £34, but only £31.4 (nearly), a difference of ten per cent. But the excess over the cost of the P. & L. $\frac{1}{16}$ was partly caused by the addition to the immersion objective of a compound front, valued at $40; deducting this, leaves the cost of the simple immersion objective, with a front of a new plan, never before used (to Mr. Tolles' knowledge), $135 = £24.04.0, (nearly,) instead of Dr. P's figure of £34.

But the price that American instruments are sold at in Boston should not be compared with the price of English instruments in London, but with their cost here. The price of P. & L's $\frac{1}{16}$th, with one front, is 16 guineas—add the duty only, nothing for freight, insurance, or other charges, and the cost here is $131.71, currency—a difference from the price of the $\frac{1}{18}$ of $3.29 only, which is more than made up by the extra expense of Tolles' *mount* over that of P. & L's. In reality, however, the opticians' price of a P. & L. $\frac{1}{16}$ here is $170, so that the economy is largely on our side.

The excess of cost of American over English instruments may be fully accounted for by the higher rate of wages of skilled labour in America. As it has been publicly charged that Tolles' prices are "enormous," and as Dr. Pigott's statements and estimates appear to confirm the charge, it is due to Mr. T. that this explanation should be as widely published.

I do not suppose that Dr. Pigott wrote by authority of Messrs. Powell & Lealand, in his suggestion that they can do better than they have done, if more money was offered them. Undoubtedly, perfection has not yet been reached, even in London, and that eminent firm may yet produce instruments better than the "objectives of 1869," not merely for the extra pay, but for the honor and reputation.

Charles Stodder.

Boston, Aug. 11th, 1873.

BEADS, OR LINES?

MANY microscopists believe and insist that all the markings of diatoms are spherical beads—some that the whole siliceous material is made up of spherical deposite. As a contribution towards the solution of the question I have to report a recent observation. I was examining a frustule of *Navicula cuspidata* with a Tolles' $\frac{1}{60}$th objective and 1 in. eye-piece = 5,000 diameters. Two sets of striæ were distinctly visible, longitudinal and transverse; but both were not and could not be brought into focus together, indicating that they were not both in the same plane. When one set was distinct the other was not. There was no appearance of beads, but of smooth, clear lines.

The grand question of the minute structure of organic forms must be settled by the use of the highest powers, and best qualities of lenses, that the optician can produce. Describing organic structure with ordinary lenses, magnifying only from three to six hundred diameters, will not answer the requirements of modern research.

Charles Stodder.

Boston.

THE STRUCTURE OF THE SCALES OF LEPISMA SACCHARINA.

FOR many years this test has been subjected to most careful and critical examination, by the most competent observers, and with the best microscopes, but, after all, the true character of its markings still remains a disputed question. These differences of opinion have evidently arisen partly from the complex nature of the markings themselves, and partly from the different conditions under which they have been seen. In this scale we have coarse ribs easily seen with a very ordinary glass; and on the other hand, delicate structures severely taxing the powers of the finest objectives in existence. This fact alone, is sufficient to account for the want of agreement, without accusing any person of being biased by a theory; while those observers who think their own instruments are the best, will continue to be satisfied with what they may happen to see, and shut their eyes to any advance.

As the microscope has been improved, our ideas of the structure of the Lepisma scale have been gradually modified, and who will now claim it to be "too easy for a test-object?"

In the order of difficulty of resolution we have—

1. The heavy, longitudinal ridges, running from end to end of the scale, and slightly projecting at the point.

2. Distinct ribs generally radiating from the quill, or curved parallel with the outline of the scale, and becoming faint in the centre and parts remote from the quill.

3. Transverse corrugations of the membranes.

4. Faint, irregular veins, branching from the diverging ridges (No. 2), generally taking a transverse direction, and, together with the corrugation, causing the spurious appearance of fine beading at their points of intersection with the ridges.

To make sure of my work on this scale, I have studied it under a number of different conditions. The observations have been conducted with monochromatic sunlight; with white cloud and lamp; with central beam and oblique; with mirror, prisms, achromatic condenser, with and without central stops; and with Wenham's paraboloid. All these methods point to the same conclusions. Following up the line of observations described by the late Richard Beck, in his most valuable contribution to our knowledge of this

subject, the same results were arrived at in regard to the appearance of coarse beading, etc., viz.: "That the interrupted appearance is produced by two sets of uninterrupted lines on different surfaces."* That the longitudinal and oblique lines are on different sides of the scale, is also plainly seen by their lying in different focal planes, under a $\frac{1}{50}$ objective. And farther, while examining a scale in fluid I have repeatedly observed air bubbles on one surface of it, confined by the longitudinal ribs, and on the other side, others bounded by the oblique ridges; and on moving the slow adjustment up and down, with the movement of the bubbles under control, they never interfere or mix with each other.† Nothing further is required to prove that these markings are actually ridges, and that they project from different surfaces of the object. The experiments of Mr. Beck settle this question.

As microscopical definition advanced, the very feeble radiating lines were noticed in the spaces between the ribs, formerly thought to be smooth. In the central portion of the test these lines are parallel with the main ribbing. They, in their turn, were seen to be uneven, and pronounced to be "beaded striæ."‡ Must this fine beading, like its shadowy predecessors, be also extinguished by intersecting cross lines, and so add one more to the long list of illusory appearances? To attempt to throw some light upon this question is the principal object of the present article.

In the first place, it is far from being a difficult feat to see this beading. Any first-class lens, from a $\frac{1}{8}$ upward, when properly handled, will display it or something very like it. The writer has found it an easy task with Wales' $\frac{1}{16}$ immersion, or even with a Beck $\frac{1}{8}$ and deep eye-piece. With Tolles' $\frac{1}{50}$ immersion the fine transverse structure indicated above is brought out, and it becomes at once evident that the small beads are indeed spurious like their big brothers, and for a similar reason.

The fine transverse marking seems to branch from the faint radiating ones, and have the appearance of a net-work of minute capillaries. Besides these there are coarser transverse waves or corrugations of the membrane. In numerous instances, air-bubbles have been observed imprisoned between the heavy ribs on one or two sides, and by these corrugations on the other sides. Therefore the corrugations may safely be said to be on the same surface

* Achromatic Microscope, Beck, p. 50. † Micrographic Dictionary, 2d ed., p. 34, Fig. 3, pl. 27.
‡ See M. M. Journal, March, 1873, pl. xi, Figs. 3 and 4.

of the scale with the longitudinal ridges, and the branching vein-like structure on or near the other surface. Careful focussing is corroborative of this idea, making it certain that these two details of structure lie in different planes. With monochromatic light, the delineation of this structure is eminently satisfactory, and the effect of the slightest change in focal adjustment is at once felt. When the object is a little out of focus the light is unequally refracted and broken up in passing through this complicated net-work of ridges and corrugations, and produces an appearance of fine molecules over the whole surface of the scale.

The coarse and the fine beads both vanishing under advancing definition, together with the behavior of the confined bubbles of air, seems to my mind fully to demonstrate the reality of the structure above described. Often, when the corrections are not perfect, the semblance of beading can directly be traced to a seeming enlargement of points of linear intersection and branching. When the $\frac{1}{50}$ is at it its best work, the finer transverse markings are usually irregular, both in strength and direction, but always unmistakable. They may be plainly seen on some of the smaller scales, and in the central parts of the larger, and at almost as good advantage as near the edges of the easier scales. Sometimes they are continuous across several intercostal spaces, and again only extending across one, or merely budding, as it were, from the ribs.

In conclusion, the remark of Beck on the scales of Lepidocyrtus, may well be quoted: "And my own belief is that the markings upon this and all other varieties of Podura-scales are more or less elevations or corrugations upon the surface, which answer the simple purpose of giving strength to very delicate membranes."* If this idea is true of the Podura, it applies with greater force to the complicated ridges of Lepisma.

The same original structure is often modified in diverging directions, so as to subserve totally distinct purposes. And as hairs are probably modified scales, and a regular graduation may be traced between them, so the connecting chain is filled between ribs extending from end to end of a scale, through undulations and shorter ribs, to those slightly projecting, and so on to the perfect spine or secondary hair.

G. W. Morehouse.

American Naturalist.

* Transactions, R. M. S., 1862, p. 83.

THE FLORA OF CHICAGO AND VICINITY.

SUPPLEMENTARY.

ANEMONE CYLINDRICA, Gray; lake shore; common.

VIOLA CANINA, L., var. *sylvestris*, Reg.; Glencoe; common.

EUNONYMUS AMERICANUS, L., var. *obovatus*, Torr. & Gray; Riverside; rare.

ACER DASYCARPUM, Ehrh.; common.

DALEA ALOPECUROIDES, Willd.; Hinsdale; a waif.

AMORPHA FRUTICOSA, L.; Hyde Park; rare.

POTENTILLA ARGUTA, Pursh; Hyde Park and Riverside; common.

RUBUS TRIFLORUS, Richardson; Calumet; rare.

R. STRIGOSUS, Michx.; Pine Station; rare.

R. OCCIDENTALIS, L.; common.

CIRCÆA ALPINA, L.; Michigan City; rare.

POLYTÆNIA NUTTALLII, DC.; Maywood; rare. (*H. A. W.*)

CORNUS CIRCINATA, L'Her.; Michigan City; rare.

VIBURNUM PUBESCENS, Pursh; Glencoe and Riverside; common.

MITCHELLA REPENS, L.; Michigan City; common.

ASTER CORDIFOLIUS, L.; Glencoe; not common.

A. LONGIFOLIUS, Lam.; Glencoe; common.

A. MACROPHYLLUS, L.; Glencoe; common.

SOLIDAGO MUHLENBERGII, Torr. & Gray; Pine Station; rare.

S. SPECIOSA, Nutt.; Pine Station; common.

S. SPECIOSA, Nutt., var. *angustata*, Gray; Hyde Park; common.

HELIANTHUS TRACHELIIFOLIUS, Willd.; Hyde Park; not common.

ACTINOMERIS HELIANTHOIDES, Nutt.; Hinsdale; rare.

PLANTAGO LANCEOLATA, L.; Hinsdale; rare.

P. CORDATA, Lam.; Glencoe; common.

TRIENTALIS AMERICANA, Pursh; Millers'; rare.

LEONURUS CARDIACA, L.; Riverside; rare.

ELLISIA AMBIGUA, Nutt.; Riverside and Hinsdale; common.

GENTIANA PUBERULA, Michx.; Hyde Park: not common.

CYCLOLOMA PLATYPHYLLUM, Moq.; I. C. R. R.; rare.

SALIX* CANDIDA, Willd.; S. W. of Hyde Park; not common.

S. HUMILIS, Marshall. The variety common on western prairies, with oblong-lanceolate, sage-like leaves; Hyde Park and S.; com-

* Determination of species of this genus, and remarks, by M. S. Bebb, Esq., of Fountaindale.

mon. Also, dwarf forms of the same, closely approximating *S. tristis*, near Kenwood.

S. DISCOLOR, Muhl.; Hyde Park, Glencoe and Maywood. (*H. A. W.*)

S. PETIOLARIS, Sm.; Maywood. (*H. A. W.*)

S. CORDATA, Muhl.; var. *rigida*, Carey; lake shore near 35th st.

S. CORDATA, Muhl.; var. *angustata*, Gray, Anders, &c.; with the last.

S. CORDATA, Muhl.; var. *glaucophylla*. A beautiful Willow, common on the lake shore. (*H. H. B.*). (Wisconsin, *T. J. Hale*. Fountaindale, very rare.) Not known to occur elsewhere. Apparently distinguishable from *cordata-rigida*, and were it not for the bad reputation which *Salix* has on account of the multiplication of supposed species we would be tempted to separate it. Resembles *S. discolor* in the shape of the leaves and in the *size* and density of the thick aments, but the capules are smooth, and turn brown in drying. Adult leaves coriaceous, shining above, *intensely glaucous* beneath; pedicels usually concealed by the copious, silky hairs of the scales. Flowers a week later (at Fountaindale) than the preceding forms. Abundant from 33d street to Cornell.

S. ADENOPHYLLA, Hook. Lake shore, (where like the associated *Corispermum*, &c., it seems to be a waif from farther north;) near Lincoln Park; (*Dr. Vasey, H. H. B., H. A. W.*) Very abunbant on the South Side, from Reform School to Pine Station. (*H. H. B.*). Lake Superior, (*Dr. H. J. Beardslee.*) "Sand beaches of Cockburn's Island, Lake Huron," (*J. Bell* in Herb. A. Gray.) This very distinct species was first described in the *Flora Borealis Americana*, from specimens collected in Labrador by Dr. Morrison, and the recent descriptions of Anderson are evidently drawn from the same material without further additions. It appears, therefore, to have been known only through some "old" female catkins and leaves, preserved in the *Hookerian Herbarium* until re-discovered near Chicago; after which the very complete collections made by Mr. Babcock led to the identification of fragments from other localities. "I know no species like this, well marked as it is by the copious, long, narrow serratures to the leaves, tipped with glands, so that the leaf looks as if it were fringed with pedicellate glands. These leaves are an inch or more long, clothed, even when fully grown, with long silky tomentum on both sides, but which is deciduous on the oldest leaves." (*Hooker.*)

S. ROSTRATA, Richardson. To be united, probably, with one of the European *Cinerascentes*, in which case the original name should be preserved for the variety, but so long as we retain *lucida* as distinct from *pentandra*, we may as well keep *rostrata* also. Hinsdale, Downer's Grove and Miller's.

S. LUCIDA, Muhl.; Hyde Park to Woodlawn and S. Specimens from Woodlawn have the scales distinctly *dentate*, as in the Rocky Mountain *S. Fendleriana!*

S. NIGRA, Marsh., var. *falcata*, Carey, &c.; Glencoe.

S. NIGRA, Marsh., var. *amygdaloides*, Anders.; S. of Hyde Park. Ordinarily appears quite distinct from *S. nigra;* but intermediate forms occur.

S. FRAGILIS, L.; I. C. R. R., South of Woodlawn. Extensively planted throughout the Northwest for screens or so-called hedges, under the name of "White Willow."

S. ALBA, L., var. *vitellina*, Koch. Introduced about dwellings, &c.

S. LONGIFOLIA, Muhl.; Hyde Park and S.; common. Here, as elsewhere from New York to California, this species produces flowers and fruit throughout the season—with us from May to September.

ACORUS CALAMUS, L.; Calumet; common.

POTAMOGETON GRAMINEUS, L.; Pine Station; common.

SAGITTARIA VARIABILIS, Engelm.; form with double flowers found at Hinsdale by Mr. C. J. Fellows.

JUNCUS NODOSUS, L., var. *megacephalus*; Torr.; Pine Station.

SCIRPUS FLUVIATILIS, Gray; Calumet; common.

FIMBRISTYLIS LAXA, Vahl.; Hyde Park; common.

CAREX UMBELLATA, Schk.; Pine Station; common.

C. LUPULIFORMIS, Sartw.; Glencoe; rare. A form intermediate betwen this and *lupulina*, Muhl.; at Riverside; rare.

PANICUM DEPAUPERATUM, Muhl.; Hyde Park; common.

H. H. Babcock.

Chicago.

EDITOR'S TABLE.

NEW RESEARCHES ON THE NATURAL HISTORY OF BACTERIA.—Dr. Ferdinand Cohn, in the second part of his *Botanical Contributions*, recently published at Breslau, gives an account of researches which he has conducted for many years, on various questions relating to Bacteria. A highly important memoir was published twenty years ago by this algologist, in which he established the occurrence of various phases in the life-history of Bacteria, especially the Bacterium-jelly or Zooglœaform. In his new work Cohn divides the Bacteria, which he now regards as colorless algæ, allied to the *Oscillariæ*, into four groups, reserving, however, with regard to these groups and their included genera, the question of their being phases of one or more real species. These groups are spherical Bacteria (*Sphærobacteria*), peg-like Bacteria (*Microbacteria*), filamentous Bacteria (*Desmobacteria*), and spiral Bacteria (*Spirobacteria*). The minute spherical organisms, little more than mere granules, which appear to be connected with certain diseases (vaccinia, diphtheria, pyæmia, pébrine), and which also cause colored putrefaction, and the alkaline fermentation of urine, appear in the first group under the genus *Micrococcus* The common *Bacterium termo* and the larger but abundant *B. lineola* belong to the second group, as well as some color-producing ferments of which that of blue-green pus is especially interesting. The butyric ferment (*Bacillus subtilis*) and the organism connected with the disease known as "malignant pustule" and "the blood" (*Bacteridium anthracis*) belong to the third group, which likewise includes the undulate forms comprised in the genus *Vibrio*. The fourth group contains the remarkable forms of *Spirillum* and *Spirochæte*, one of which, *Spirillum volutans*, is provided at each end with a protoplasmic flagellum. The typical forms are all clearly figured in a plate. Cohn considers that the protoplasm of Bacteria is not naked, as sometimes supposed, but that it has a dense cell-wall. Bacteria multiply exclusively by transverse fission, and never branch; by arrest of the actual separation of new-formed cells or cytods (for there is no nucleus) they may, however, form chains, or grow into long filaments, in which the division into separate elements cannot be recognized (*Leptothrix* forms). They exhibit very active movements in the presence of oxygen, but become quiescent in its absence. After an exhaustion of the nutriment or the supply of oxygen accessible to them in an infusion, they form a fine precipitate, and remain in this state, preserved from decomposition by their dense cell-walls; this is also the case if they are boiled or treated with reagents. According to their external effects Bacteria may be classed as Chromogenous, Pathogenous, and Zymogenous or Saprogenous. The color-producing Bacteria are of very great interest. Their characters have been

traced out, to some extent, by Schröter, under Cohn's direction. One form especially, *Micrococcus cyaneus*, Cohn found could be transferred from the boiled potato on which it made its appearance, and cultivated in a one *per cent.* solution of tartrate of ammonium, with a proportion of yeast-ash salts. Under these circumstances the production of intensly blue soluble pigment continued for some months. Sanderson had already used Pasteur's solution for the cultivation of Bacteria, but Cohn found it better to omit the sugar from the solution. The Bacteria, however, absolutely failed to grow if the yeast-ash salts were omitted. As Bacteria are thus found to be capable of taking up their nitrogen from ammonia, Cohn considers it probable that they always receive it in this form, or perhaps sometimes from nitrates, and that the chief work of the putrefactive Bacteria consists in breaking down complex organic molecules containing nitrogen to the condition of ammonia. Their carbon, it appears, must be in a higher condition of combination than is met with in carbonic acid, and in this respect only does their nutrition differ from that of green plants. Their vital processes, like those of all protoplasm, are necessarily accompanied by the fixing of oxygen and the evolution of carbonic acid. The specific products of their life-activity, such as pigments, foul gaseous substances, &c., are independent of the chemical changes in their pabulum and are due to internal chemical work, which goes on just the same, whether they are nourished by organic infusions or by ammonium tartrate.

PHOTOGRAPHY OF THE INVISIBLE.—We cut from a late issue of our esteemed co-worker, the *Scientific American*, the following methods of taking the so-called "spirit photographs":

The grand moral idea which science continually seeks to impress upon her votaries is, humility of mind; that inestimable virtue whence spring the noblest pleasures of the soul. But how rare it is to find this beautiful quality, even in persons of culture and learning! The great doctors looked upon Galileo with contempt, confined him in prison as a dangerous man, and subjected him to the most ignominious treatment, simply because he presented, for their acceptance, the light from a new idea, which their dull perceptions were unable to appreciate. He affirmed that the sun did not really rise or set; that it was the rotation of the earth that brought day and night alternately upon the earth. But the doctors, like many in our day, proud in their own conceit of knowledge, knew better. "The scriptures tell us," they said, "of the rising and the setting sun; therefore it moves; our own eyes assure us of the fact; the diurnal experience of mankind confirms the truth. Your doctrine, Galileo, is false and dangerous."

It is in this style that some persons, very knowing in their own esteem, reason upon certain subjects. Take "spirit photography" for an example. They allege that spirits are invisible; that an invisible thing cannot be photographed; therefore the so-called spirit photographs are base impostures.

It is not our purpose to dissent from the conclusion here assumed; but we take exception to the premises, which are not in agreement with science. Photographs of some objects that are invisible to the human eye may undoubtedly be produced.

The spectrum of solar light is an example, portions of which, totally invisible to the eye, are brought out upon the photographer's plate; and their presence is also demonstrated by other instruments.

The mental effect which we term light is supposed to be produced by the beating of waves of ether against the retina of the eye. These waves enter the eye with an average velocity of about 186,000 miles in a second, the length of the waves being variable, from the one twenty-seven thousandth part of an inch, to one seventy-five thousandth part of an inch. The retina therefore receives many billions of impressions in a second, and it is supposed that it is the difference in the number and velocity of these impressions that produces in the mind the sensations of the colors. If the waves which enter the eye have a much greater or a much less velocity than the limits above stated, they do not, it is supposed, produce the sensation of light; and the objects from which such rays come, although they may really stand before the eye, are, as we say, invisible. But although they do not affect the eye, they may impress the photographic plate, which has no such constitution as the eye.

One of the most successful methods of producing spirit photographs is to place, in front of the sensitive plate, within the plate shield, a clear sheet of glass having nothing upon it except a thin positive of the "spirit" that is to be produced on the negative. The portrait of the sitter is taken in the usual manner. The light which enters the camera lens prints the sitter and also the "spirit" which is on the thin positive upon the negative. This is a very convenient method, as it requires no manipulations likely to be detected; and is, we think, the favorite plan practiced by the best spirit photographers. Prints made in this manner pass current among the believers for genuine ghosts of the departed, directly descended from heaven.

But a more new, interesting, and scientific method of producing spirit photographs, is as follows: the plain background screen, before which the sitter is placed in order to have his portrait taken, is to be painted beforehand with the form of the desired "spirit," the paint being composed of some fluorescent substance, such as a solution of sulphate of quinine. When this painting dries on the screen, it is invisible to the eye; but it sends out rays that have power to impress the photo-plate; and thus the image of the person together with the quinine ghost are simultaneously developed upon the negative. This is a very beautiful and remarkable method.

A NEW THERMOMETER.—*Les Mondes* describes a maximum and minimum thermometer formed of a compound spiral, consisting of two differently expansible metals electro-plated. By increase of temperature it tends to unroll, by decrease to twist up; in so doing it moves one or the other of a pair of indices over a graduated scale, the one of which registers the highest temperature attained, the other the lowest. If required, a third needle may be added, by means of which the actual temperature at any moment may be read off without disturbing either of the others. The instrument is used at several meteorological stations in Switzerland and Russia, and gives great satisfaction.

IS CARBOLIC ACID A FAILURE.—Most questions have two sides, and it is wise to look at both. While we have been disinfecting with carbolic acid, chlorine and coal gas, and fumigating with burned tar and sulphur, Jerome Cochran, M. D., professor of hygiene and medical jurisprudence, in the medical College of Alabama, and censor of the State Medical Association, seriously questions if there be any disinfectant virtue in those crude materials of our sanitary regulation. Professor Cochran writes nearly four columns in the *Mobile Register*, of a late date, on this subject, and fortifies himself behind some stubborn facts. He is evidently well read in the subject whereof he treats. After reviewing the action of the Mobile Board of Health, which contended that the comparative exemption from yellow fever in that city was due to this disinfecting agency of carbolic acid, Professor Cochran says there is not a particle of reliable evidence to show that they have derived any benefit at all from all the carbolic acid scattered in their streets and yards. He contends that if carbolic acid has any power to destroy the infectious germs of yellow fever, it ought to exhibit that power most clearly where it has been most freely used. He shows that the City Hospital of Mobile has been more thoroughly disinfected than any other part of the city; that the whole atmosphere in the vicinity has been saturated with it for weeks, and yet the protective virtues of disinfection have failed to check the progress of yellow fever in the hospital and vicinity, but have also failed to modify its type, while at other places in the city where disinfectants were not used there was no fever. He claims that in the experience of Mobile, time and money have been thrown away in the use of disinfecting agents.

Not only do the facts and examples adduced in proof fail to establish the efficacy of carbolic acid as a prophylactic against yellow fever; but without any violence and without any sophistical interpretation, they go very far toward the establishment of the suspicion that its influence has been the very reverse of prophylactic; that if it has not contributed to increase the extension of the disease, it has at least added to the malignity and increased the mortality to a fearful rate.

He goes on to say that it is pertinent to inquire whether any part of the mortality in New Orleans and Mobile may not be due to carbolic acid. Further, he says with reference to the use of carbolic acid:

"The experiment has been made and has failed; and it is due to the cause of truth and sanitary science, that no false and misleading estimate of what it has accomplished should be allowed to fasten itself on the public mind."

So here we are at sea. And where are we now for safety? Clearly, in our own acts, in the purity of our houses and premises, for beyond and above the theories of learned doctors, who never did agree, is that safe guide of conduct, the experience of mankind, which supplements the Scriptural truth, that cleanliness is next to Godliness, and Godliness is the twin sister of good health.

Special attention is called to Dr. Woodward's exhaustive paper on the *Webb Test*, with the Woodburytype plate accompanying it, which with the other papers herein, brings the subject to its latest phase.

THE STUDY OF NATURE AS A MEANS OF INTELLECTUAL DEVELOPMENT.—We find in the *Rhode Island Schoolmaster* the following excellent remarks on this subject:

Some affirm that the study of natural science is fatal to the development of our higher emotions, and tends towards gross utilitarianism. But who can study the harmony existing in the works of Nature, the manifest order and design displayed in endless changes and variety, and the immutable laws which govern the physical world, without having his thoughts and aspirations lifted to Him who inhabiteth eternity, the Alpha and Omega? "The heavens declare the glory of God! Day unto day uttereth speech, night unto night showeth knowledge!"

Astronomy writes, in the motions of the stars, poetry more glowing than human pen ever produced. Botany leads us among the flowers, the most unpretending of which is arrayed in glory greater than that of Solomon, and teaches Divine goodness and love to every thoughtful observer. Chemistry, unfolding to us wonderful and mysterious changes, excites not only emotions of beauty but of sublimity. And what shall we say of that marvellous agent, vital force, which still eludes the analysis of the latest science? In autumn it withdraws its power and all Nature is clad in the habiliments of decay and death. In the spring time, with magic hand, it robes the earth in living beauty.

Adding, to a thorough knowledge of any one science which might be chosen as a particular field of research and study, a knowledge of the most important principles of the others, we have sufficient matter for the development of the most susceptible and retentive memory.

By constantly observing facts, drawing conclusions from them, and verifying these conclusions by observation or experiment, we form the habit of correct reasoning, and thus gain the same kind of discipline which geometry or any other abstract science affords. Nor is discipline alone the result of the study of Nature as is often the case in absolute sciences. Nature rewards her students not only with discipline but with knowledge the most practical, pleasurable and profitable.

A NOVEL AND SIMPLE ELECTRIC LIGHT.—Dr. Geissler, of Bonn, Germany, whose name is inseparably associated with some of the most beautiful experiments that can be performed by the agency of electricity, makes an electrical vacuum tube that may be lighted without either induction coil or frictional machine. It consists of a tube an inch or so in diameter, filled with air as dry as can be obtained, and hermetically sealed after the introduction of a smaller exhausted tube. If this outward tube be rubbed with a piece of flannel, or any of the furs generally used in exciting the electrophorus, the inner tube will be illumined with flashes of mellow light. The light is faint at first, but gradually becomes brighter and softer. It is momentary in duration: but if the tube be rapidly frictioned, an optical delusion will render it continuous. If the operator have at his disposal a piece of vulcanite, previously excited, he may, after educing signs of electrical excitement within the tube, entirely dispense with the use of his flannel or fur. This will be found to minister very much to his personal ease and comfort. He may continue the experiments, and with enhanced effect, by moving the sheet of vulcanite rapidly up and down at a slight distance from the tube. This beautiful phenomenon is an effect of induction.

DESMIDS.—A correspondent asks whether Desmids can be mounted for the microscope. There are several media recommended for the purpose, but none of them are quite satisfactory. Probably the best medium is distilled water with a little camphor. Distilled water, 13 parts; gum-arabic, 1 part; glycerine, 1 part, answers well; but the delicate green color of many species fades or changes to brown sooner or later, and a more serious change still, due to alteration in endochrome, also occurs, unless the fluid, if other than water, be added very cautiously. Glycerine jelly does not answer well unless very skilfully used. Farrant's medium I have not used for this purpose without modification. The original recipe is 1 fluid oz. of the best gum-arabic in the same quantity of glycerine, with an ounce of distilled water, in which 1½ grain of arsenic has been previously dissolved. The solution must be made without heat, the mixture being occasionally stirred gently but not shaken, and if necessary, carefully strained when completed. I have used a very similar preparation in mounting delicate preparations, with success, and have no doubt our correspondent will find it answer his requirements. He will, of course, take care to transfer his desmids into it by easy stages from the native water. He would better allow them to remain for some time in distiiled water, and add glycerine or pure sugar to it, by small quantities at a time, until the fluid has assumed the same density as the medium. I allow the desmids to remain in it at this stage for some time, and then transfer them to the Farrant, taking care to use it at as low a temperature as possible.

DISINFECTION OF AIR OF SICK ROOM.—The three best agents for accomplishing the disinfection of air after smallpox or other contagious diseases, are sulphurous acid, iodine, and carbolic acid. The best method of employing sulphurous acid is to scatter a little sulphur upon a heated shovel and carry it about in the room or rooms which are to be disinfected.

Iodine may be used by simply placing a little in an open glass or earthen vessel, and it vaporizes readily at the ordinary temperature of a house. Carbolic acid may be employed by sprinkling a weak solution of it on the floor of the room, or cloths wetted in such solution may be hung about the rooms. A simple apparatus for using this acid is to have a broad band of cotton passing over two wooden rollers over a dish filled with a solution of the acid. As the upper half of the band dries, give the rollers a turn, and the lower half of the band, wet with the solution, takes its place uppermost.

DIAMONDS IN CALIFORNIA.—Professor Silliman has recently called attention to the probable occurrence of small diamonds in the sands left in the sluices of hydraulic washings in California. A microscopic examination of a sample of these sands from Cherokee, in the Butte county, revealed the existence of numerous crystals of hyacinth or zircon, associated with crystals of topaz, fragments of quartz, black grains of chromite and titanic iron ore, and a few small masses of a highly refracting substance, which, from its physical and chemical characters, is believed to be true diamond.

ORIGIN OF THE BOTANICAL NAME ANDROMEDA.—Botanists, says the *Gardener's Chronicle*, are frequently taxed with the want of euphony and of poetry in the Plant Names which they bestow; and it must be admitted that many fearful "jawbreakers" might be sighted in support of the charge. Occasionally, however, we find names bestowed in a more romantic spirit; and such is the case with the *Andromeda*, a title which Linnæus first bestowed upon the British example of the genus, *A. polifolia*. In his *Tour in Lapland* he tells us of the connection between the flower and the heroine of mythology which led to his selection of the name:

"As I contemplated it, I could not help thinking of Andromeda, as described by the poets; and the more I meditated upon their descriptions, the more applicable they seemed to the little plant before me; so that had these writers designed it, they could scarcely have contrived a more apposite fable. Andromeda is represented by them as a virgin of most exquisite and unrivalled charms; but these charms remain only so long as she retains her virginal purity, which is also applicable to the plant now preparing to celebrate its nuptials. This plant is always fixed on some little turfy hillock in the midst of the swamps, as Andromeda herself was chained to a rock in the sea, which bathed her feet, as the fresh water does the roots of this plant. Dragons and venomous serpents surrounded her, as toads and other reptiles frequent the abode of her vegetable resembler, and when they pair in the spring, throw mud and water over its leaves and branches. As the distressed virgin cast down her blushing face through excessive affliction, so does this rosy colored flower hang its head, growing paler and paler till it withers away. . . . At length comes Perseus, in the shape of summer, dries up the surrounding water, and drives away the monsters, rendering the damsel a fruitful mother, who then carries her head (the capsule) erect."

CAMPHOR A DANGEROUS DRUG.—The *Scientific American* lectures those people who make frequent use of camphor as a medicine. It says: The physiological action of camphor is not yet understood; but, judging by the symptoms that follow the taking of a moderate dose, it may be called a nervous stimulant. It is somewhat like opium and alcohol, therefore, in its action when given in small quantities; but, when taken in large doses, it causes excessive irritation to the nervous system, producing convulsions and death. Camphor acts to irritate and congest and finally to inflame the mucous lining of the stomach, causing in the milder cases a form of dyspepsia, and, in the more aggravated, ulceration of the stomach. From these two actions, namely, that of the nervous stimulant and local irritation, come all the good and evil of its use. We can, therefore, readily see how unsuited this drug is to be a household remedy.

CEMENTING METAL TO GLASS.—Take two parts finely powdered white litharge, and one part dry while lead, mix intimately, and work up with boiled linseed oil and lac copal to a stiff dough. One part of copal is taken to three parts boiled oil, and enough litharge and white lead added to make dough similar to putty. The underside of the metal is filled with the cement, and then pressed upon the glass, the excess of cement being scraped off with any sort of instrument. It dries quickly and holds firmly.

LUMINOUS FUNGI.—In Mrs. Somerville's *Molecular and Microscopic Science* it is stated that "In the dark coal-mines at Dresden luminous fungi cover the roof and pillars with the most dazzling phosphorescent light, which increases with the temperature of the mine."

Now it seems to me that this statement, if not entirely unfounded, at least requires some qualifications; and small errors of this kind should, I think, in the interests of your many readers, not be allowed to pass unnoticed. I myself have, on several occasions, visited these mines—which, by the way, are not actually in Dresden, but are situated near Plauen, some five miles distant—and have most carefully examined these so-called luminous fungi. The result of my observations was, that under no circumstances was the faintest luminosity, even in complete darkness, to be detected.

The fungi—locally known as "*schlamm pilz*"—occur in great abundance in the deeper workings of the mines; they are in appearance purely white, and have an exquisitely delicate structure, but, from all accounts, do great injury to the timbers and supports. In form they are usually sponge-shaped, and attached to the substance on which they grow by a single stalk or filament. A damp atmosphere seems essential to their existence, and where the ventilation was good their appearance was less frequent.

It is my impression that what has led to the belief in their luminosity is simply their remarkable, almost absolute, whiteness, which in a faint light makes them stand out conspicuously from the dark roof and walls of the mines. Extremely small crystalline spines of saltpetre occasionally cover the face of the rock, and give a slightly sparkling reflection when passing with a lamp; this may possibly have helped to create the illusion. As to applying the term "dazzling" to the fungi themselves, it is, I should say, extremely misplaced. A. W. R.

PREPARING PATHOLOGICAL SPECIMENS FOR TRANSMISSION BY POST, &C.—Dr. J. C. Richardson, microscopist to the Pennsylvania Hospital, publishes a paper in the Philadelphia *Medical Times* on a new method of preserving tumours and certain urinary deposits during transportation; and as the process is applicable to other than pathological specimens, we abstract a portion of the article for the benefit of such of our readers as may be engaged in histological work. The chemical made use of has been recommended as a preservative medium in these columns on several occasions. Place a small fragment of any tumour or pathological structure, say ¼ to ½ an inch square and 1-10th of an inch thick, in a couple of drachms of saturated solution of acetate of potash, and allow it to fully imbibe the fluid by soaking therein for forty-eight hours. The solution is best made by simply pouring half an ounce of rain water upon an ounce of dry granular acetate of potash in a clean bottle. When the tissue is fully saturated with this saline liquid, remove it with a pair of forceps without much pressure, and insert in it a short piece of india rubber tubing, or wrap it up carefully in a number of sheets of thin sheet rubber or oiled silk, tying the whole firmly at the ends with stout thread. When thus prepared, specimens can be inclosed with a letter in an ordinary envelope and sent long distances, doubtless thousands of miles, by mail, without danger on the one hand, of decomposition, because of the

preservative nature of the potassium acetate, or, on the other, of desiccation on account of its exceedingly deliquescent nature. Inorganic urinary deposits cannot, the author remarks, be preserved by this agent, but fatty and similar preparations are peculiarly suited for it. It is worth while to recall to our readers' recollection that the potassium acetate is singularly adapted for all objects stained with osmic acid, and all tissues containing fat cells or oil globules.

INK IN ADULTERATED TEA.—One often hears of an article, pamphlet, or other written statement, "carrying its own proof on the face of it," but the expression generally means no more than that its arguments are strikingly forcible and unanswerable. The phrase applies, however, in a new and more literal sense to the article which Dr. Hassall has communicated to a late number of *Food, Water, and Air*, on the adulteration of tea. Dr. Hassall states his belief that iron filings are added to tea less for the purpose of increasing the weight and bulk than for giving it a dark complexion; and he adds that since tea naturally contains a large quantity of tannin, there are thus brought together the two chief constitutents which enter into the composition of ink. In order to place this point beyond a doubt, Dr. Hassall has actually, by appropriate treatment, extracted a bottle of ink from the tea in question, and has written with it a portion of his article. Under these circumstances, undoubtedly, Dr. Hassall's article becomes a piece of (in every sense) powerful writing in support of the virtual identity of ink and tea, and only very determined opponents of his conclusions will venture beyond a merely superficial examination of his paper. There is, however, an unpleasant interest attaching to his further surmise that "what has been accomplished in the laboratory it is not impossible may arise in the human stomach, into which largely-adulterated iron-filing tea has been received." Pending the enaction of an adulteration act, grocers might be kind enough to supply the antidote with the bane by selling us our tea wrapped up in blotting-paper.

THE GERM THEORY.—An article in the *Journal of Applied Chemistry* collects the following curious facts: "Unfortunately, the results as to how organic matter is rendered harmless by heating are contradictory. It is known that living organisms exist in the hot springs of Iceland, where the temperature is 97.8 degrees C —209 degrees F. Cohn says that boiling for a short time, or even heating to 80 degrees C., is sufficient to prevent the generation of bacteria. According to Hoffmann, bacteria are destroyed only by boiling a long time in open vessels, or by heating a short time to the boiling point in sealed glass tubes. Wyman found that it required five or six hours' boiling to destroy the last germ. Pasteur asserts that these organisms are not killed below a temperature of 110 degrees C., and Lex found that they were still alive after heating for a short time to 127 degrees. Grace-Calvert obtained still more remarkable results, for he found that they were not killed below a temperature of 240 degrees Centigrade. Forster communicates the fact that contagion has been spread through water that has been boiled. Even if boiling is not a perfect protection, it must be admitted that most of the germs of putrefaction and fermentation are rendered harmless by the boiling heat, but it is safer, after boiling, to add permanganate of potash.

THE OPEIOSCOPE.—This is a new and simple instrument, suggested by Professor A. E. Dolbear, for the purpose of demonstrating the pulsations of sound. Take a tube of any material, from one to two inches in diameter, and anywhere from two inches to a foot or more in length. Over one end paste a piece of tissue paper, or a thin piece of rubber or goldbeater's skin — either will do. In the centre of the membrane, with a drop of mucilage, fasten a bit of looking-glass not more than an eighth of inch square, with the reflecting side outward, of course. When dry, take it to the sunshine, and, with the open end of the tube at the mouth, hold the other end so that the beam of reflected light will fall upon the white wall, or a sheet of paper held in the hand. Now speak, or sing, or toot in it. The regular movement of the beam of light with the persistence of vision, presents very beautiful and regular patterns, that differ for each different pitch and intensity, but are quite uniform for given conditions. If a tune like "Auld Lang Syne" is tooted slowly in it, care being taken to give the sounds the same intensity, a series of curves will appear, one for each sound and alike for a given sound, whether reached by ascension or descension, so that it would be possible to indicate the tune by the curves; in other words, it is a true phonautograph.

By trial one can find some tone which causes the membrane to vibrate in a single plane, and of course a straight line will appear upon the screen. If, while the sound is continued, the tube be swung back and forth at right angles to the line, the sinuous line will appear, which may be either simple, representing a pure and simple sound, or it may be compound-sinuous, showing over-tones, precisely as in Kŏnig's manometric flames.

With the lecture-room darkened and using the beam of light from a *port lumière* or from a lantern, these may be projected of an immense size. There is no trouble in the world in making them eight or ten feet amplitude or more if needed. At a distance of but three or four feet, the curves will spread out to two or three feet in length when a tone is made to which the tube can reasonably respond.

THE ABSORPTION BANDS OF CHLOROPHYLL have been studied by M. Chautard, who divides the chlorophyll bands into three distinct categories. The first contains simply the band in the middle of the red; this he calls the *specific band.* In the second he includes all bands which have been observed in chlorophyll solutions, new or old, neutral, acid, or alkaline; these he calls *supernumerary bands.* The most remarkable is that which results from division of the specific band in the red, under the influence of alkalies. The third category comprises *accidental bands,* not having the permanent character of the preceding, and being produced in special conditions. Of this kind is that from a division of the specific band through acids. The additional band here seems to arise from the less refrangible side, while in the alkaline solution it arises from the other. M. Chautard gives full particulars of the treatment of chlorophyll to obtain various bands.

Our next number will contain the conclusion of Dr. Barnard's paper on the Germ Theory, and the two papers by M. Nobert from *Poggendorff*, referred to by Dr. Woodward, on page 227 of this issue.